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AND

JAMES D. DANA,

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ERRATA.—P. 178, 19th l. from top, for 1838, *read* 1853.—P. 274, for "Nieder, Pobel," *read* "Nieder-Pobel."

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THE

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[SECOND SERIES.]

Art. I.—*The Smithsonian Institution.*

We have been silent but not uninterested spectators of the controversy which has been so rife for a year past respecting the Smithsonian Institution. Now, however, when the discussion appears to have drawn to a close, or at least when the arguments on both sides are well nigh exhausted, it seems proper, in view of the relations which this Journal sustains to science and to the public, that we should take this important subject into dispassionate consideration, and place our conclusions upon record.

The official documents with which we have to do, are:

1. The will of the founder, James Smithson.
2. The Act of Congress entitled "An Act to establish the Smithsonian Institution for the increase and diffusion of knowledge among men," passed in August, 1846.
3. The eight annual Reports of the Board of Regents of the Institution, made to Congress, from 1847 to 1854.

All these are included in the Eighth Annual Report of the Board, showing the operations, expenditures, and conditions of the Institution up to Jan. 1, 1854, and the proceedings of the Board up to July 8th, 1854.

And further, as to the points in controversy:

4. Report of the special committee of the Board of Regents, Hon. Mr. Pearce, Chairman, relative to the distribution of the income of the Smithsonian fund, made May 20, 1854, published in the Proceedings of the Board, forming a part of the eighth annual Report, above mentioned.

5. The Hon. Mr. Meacham's minority Report from the above committee, made subsequent to the adjournment of the Board of Regents and the presentation of their annual Report to Congress, but appended to the House of Representatives' edition of said Eighth Annual Report.

6. The Proceedings of the Board of Regents at the annual meeting in January, 1855;—an abstract from the records, published in the National Intelligencer of Jan. 17th, 1855.

7. A letter of the Hon. Rufus Choate to the President of the Senate and the Speaker of the House of Representatives, resigning his office of Regent of the Smithsonian Institution; and the Proceedings in the Senate consequent thereon. (Congressional Globe, &c.)

8. Report of the Judiciary Committee of the Senate, to whom was referred the inquiry whether any, and if any, what, action of the Senate is necessary and proper in regard to the Smithsonian Institution; made Feb. 6, 1855.

9. Speech of the Hon. W. H. English in the House of Representatives, Feb. 27, 1855, on the Smithsonian Institution.

10. Report of Hon. Mr. Upham, Chairman of the Select Committee of the House of Representatives, to which was referred the letter of the Hon. Rufus Choate resigning his place as a Regent of the Smithsonian Institution, with instructions to enquire and report to the House whether the Smithsonian Institution has been managed and its funds expended in accordance with the law establishing the Institution, &c. &c. (Printed in the Boston Daily Advertiser, March 10; the official publication not yet issued.)

11. Hon. Messrs. Witte and Taylor's Report from the same committee. (Printed in the Boston Daily Atlas, March 16th.)

These documents all are, or readily may be, accessible to our readers in this country; and we are obliged to presuppose on their part a full acquaintance with them. However desirable a complete analysis of them might be, and however needful for the information of our foreign readers interested in a question of more than American concernment, our narrow limits forbid this, —forbid even that comprehensive historical view of the character and operations of the Institution which we would gladly give, and require us, after thus indicating the official documents which comprise the whole case, to enter at once upon the consideration of the essential points of this unhappy contest.

This controversy has grown out of certain widely divergent views which are entertained respecting the design and functions of the Institution. One party, that opposed to the policy actually adopted, contends that the Institution is by law, and should be made in fact, a *library*;—that from its income, now of about $31,000, an average amount of $25,000 should be annually expended for books. Other specified objects may subordinately or contingently come within the scope of the Institution; but the library, with its "legal possession of twenty-five thousand dollars a year" is "the paramount interest of the Institution," or would be, "had the Institution been made, as Congress intended, almost

entirely a library."* This is the view taken by Messrs. Choate and Meacham, of the Board of Regents. Another party, comprising a large majority of the present Board, maintains that a library is not the end or purpose, nor *an end* of the Institution, but is one of the instruments or means by which the objects of the establishment are to be subserved ;—these objects being "the increase and diffusion of knowledge among men," as expressed in the will of the founder, and affirmed in the act of Congress which established the Institution and prescribed the way in which they are to be attained. Of the large majority of the present managers of the Institution, coinciding in this general view, some would give the library more, others less prominence: but they all agree in principle as to their duties and obligations under the law,—maintaining as they do, in opposition to the first-named party, that they are not required to vote an annual appropriation of about $25,000 for the library, nor of anything like that amount, except when in their judgment it may be most expedient. In fact, to use the language of one of the number, they are "opposed to making the library *the principal or controlling* feature of the Institution," but "regard it as *one* of the important instruments to be used in accomplishing the desired end."†

This latter view has constantly prevailed in the Board of Regents charged with the management of the Institution,—very largely prevailed, we may add; for most of those who have favored the amplest appropriations ever proposed for the library, have evidently done so in the exercise of a discretion over the amount, which they understood to be conferred upon them by the law. By good majorities the Board of Regents have directed the action and operations of the Institution so as not to make the library "the paramount interest" or controlling feature of it, but only one among several means or instruments of increasing and diffusing knowledge. Hence the controversy, which has been carried on with so much spirit and feeling, in the Institution and elsewhere.

The voluminous minority Report of Mr. Meacham (Doc. No. 5, supra), and the able and eloquent arguments of Mr. Choate having failed to induce the Board of Regents to change their course, an appeal, in effect, was made to Congress, in a letter of resignation of his office of Regent by Mr. Choate ;—the result of which is to be found in the discussion which arose thereupon in the Senate (Doc. No. 7); in the Report of the Judiciary Committee of that body (Doc. No. 8)—chosen from among its ablest jurists,—unanimously approving the action of the Board of Regents ; and in the two reports from the select investigating com-

* Report of Mr. Meacham ; document No. 5, in the enumeration given above.

† Hon. Mr. English. Speech in the House of Representatives; document No. 9, supra.

mittee raised in the House of Representatives; that of its Chairman, Mr. Upham (Doc. No. 10), disapproving, that of Messrs. Witte and Taylor (Doc. No. 11), fully approving the course pursued by the Regents.

The questions at issue, it will be seen, are two, which should not be confounded, as they generally have been, at least in the popular discussions of the subject. The charges brought against the Board of Regents and their Secretary, when examined, are found to be: 1st, that they have *improperly exercised the powers conferred upon them*:—a charge which may involve nothing more than a difference of opinion as to the best means for attaining the desired ends; or it may impeach their judgment, or their motives. 2d, They are also charged with having *disregarded and violated the law* from which their powers and their existence are derived. This much graver charge involves a legal question,—the one upon which the gist of the controversy turns. If the questioned action of the Board is illegal, it matters little whether it would otherwise be expedient and judicious,—whether it would or would not tend most effectually to further the object in view. If, on the other hand, it should prove to be warranted by the law, we should have only its wisdom and propriety to consider.

We have heard the charges: let us now look to the law.

The purport of the law, as its preamble declares,* is to accept the trust created and proffered by the testator, and to found an establishment for "the faithful execution of said trust according to the will of the liberal and enlightened donor."

The whole of Smithson's will which relates to this bequest is comprised in the words; "I then bequeath the whole of my property, subject to the annuity of £100 to John Fitall, and for the security and payment of which I mean stock to remain in this country, to the United States of America, to found at Washington, under the name of the Smithsonian Institution, an establishment for the increase and diffusion of knowledge among men."

The "act to establish the Smithsonian Institution," consists of the preamble cited in the note below, and of eleven sections.

The first constitutes the *Establishment*,—a corporation in fact, with perpetual succession, but without the corporate power of suing and being sued,—to consist of ten specified high officers of the United States *ex-officio*, of the Mayor of Washington, and of such persons as they may elect honorary members.

* "James Smithson, Esq., in the kingdom of Great Britain, having by his last will and testament given the whole of his property to the United States of America, to found at Washington, under the name of the Smithsonian Institution, an establishment for the Increase and Diffusion of knowledge among men; and the United States having by an act of Congress received said property, and accepted said trust, therefore, for the faithful execution of said trust according to the will of the liberal and enlightened donor, Be it enacted," &c.

The second section appropriates the accrued interest of the bequest, or so much of it as may be required, for the erection of suitable buildings, and for current incidental expenses; and funds the capital, for the perpetual maintenance of the Institution.

The third section creates a Board of Regents to conduct the business of the Institution, fixes the mode of appointment of the elective members of the Board (the *ex-officio* Regents being the Vice President and the Chief Justice of the United States, and the Mayor of Washington), as well as their term of service, authorises the Board to appoint a Secretary of the Institution, who is likewise to be the Secretary of the Board of Regents, prescribes the manner of transacting business, and requires an annual report to Congress.

The fourth section directs a building to be erected.

The fifth relates to the site and character of the building; and requires it to have "suitable rooms or halls for the reception and arrangement, upon a liberal scale, of objects of natural history, including a mineralogical and geological cabinet; also a chemical laboratory, a library, a gallery of art, and the necessary lecture-rooms.

The sixth section bestows upon the Institution objects of art and natural history belonging to the government.

The seventh section enacts; "that the Secretary of the Board of Regents shall take charge of the building and property of said Institution, and shall, under their direction make a fair and accurate record of all their proceedings, to be preserved in said Institution; and the said Secretary shall also discharge the duties of librarian and keeper of the museum, and may, with the consent of the Board of Regents, employ assistants; and the said officers shall receive for their services such sums as may be allowed by the Board of Regents, to be paid semi-annually on the first days of January and July; and the said officers shall be removable by the Board of Regents whenever, in their judgement, the interests of the Institution require any of the said officers to be changed."

Section 8, enacts that the members and honorary members of the Institution (meaning thereby the establishment constituted by the first section) may hold stated and special meetings, "for the supervision of the affairs of said Institution, and the advice and instruction of said Board of Regents;" and continues:—

"And the said Regents shall make, from the interest of said fund, an appropriation not exceeding an average of $25,000 annually, for the gradual formation of a library composed of valuable works pertaining to all departments of human knowledge."

Section 9. "And be it further enacted; that of any other moneys which have accrued, or shall hereafter accrue, as interest upon the said Smithsonian fund, not herein appropriated, or not required for the purposes herein provided, the said managers are

hereby authorised to make such disposal as they shall deem best suited for the promotion of the purpose of the testator, anything herein contained to the contrary notwithstanding."

Section 10, requires those taking copyrights to deliver a copy of the publication to the Librarian of the Institution, and another to the Librarian of the Congress library.

Section 11, and last, reserves the right of altering, amending, adding to or repealing the provisions of the act.

The Smithsonian Institution, thus founded, is required by the fifth section *to have* a building suitable for its purposes, lecture-rooms, and, by necessary implication, a museum of natural history, including a geological and mineralogical cabinet, a chemical laboratory, a library, and a gallery of art. But what is the Institution *to do*? Does the mere custody of these instrumentalities accomplish the object of the trust? Or is it through their use that the object in view, viz, "the increase and diffusion of knowledge among men" is to be furthered. And if the latter, then by whom furthered? Will the mere access of the public to the books and cabinets (implied, we may well suppose, but nowhere even indirectly alluded to or provided for) increase and diffuse knowledge among men to the greatest degree, or even largely. Who is to use the lecture-rooms or the chemical laboratory and other means of research, by the mere inspection of which small increase of knowledge can be expected? Manifestly the law contemplates the fulfillment of the objects of the trust in the use of these collections, laboratories, and books, not alone by all comers promiscuously (under needful regulation), according to their various ability, but also, (and that not subordinately,) by the *Institution itself*, through action of its own, and agencies which it may set in operation;—modes of action nowhere prescribed in the law, indeed, being wisely left to the foresight and discretion of the Board, supervised by the Establishment, but for which an ample charter is conferred by the ninth section. The implication seems to be as direct in these respects, as that the ordering of lecture-rooms implies the delivery of lectures.

And how are the operations and the income of the Institution to be distributed among the several specified objects, and others deemed "best suited for the promotion of the purpose of the testator," which the managers are authorised to make appropriations for? All this, it appears to us, is plainly left to the wise discretion of the managers of the Institution, subject only to the limitation contained in the eighth section, fixing a certain but very high maximum limit to the sum which may be expended on the library.

The interpretation of this clause, however, is the turning point of the controversy. The Board of Regents have all along assumed this clause, directing an appropriation not exceeding an

average of $25,000 annually, for the gradual formation of a library, to limit their otherwise full discretion in one direction only,—to limit it upwards, not downwards. Their course of action from the first shows this. However diverse the views that have been entertained in the Board as to the propriety of large or small appropriations for the library, we are not aware that this full power and responsibility has been denied until recently. The report of a committee on the library (consisting of Messrs. Choate, Hawley, and Rush) appointed at the first session of the Board of Regents, declares, "that they see in the language of the act which the Regents are created to administer, and in the history of the passage of that act, a clear intimation that such a library was regarded by Congress as prominent among the more important means of increasing and diffusing knowledge among men. This intimation they think should control in a great degree the acts of the Regents." "And, without pausing to inquire what precise average amount should be expended, the committee will say that they have become satisfied that there would be no difficulty in judiciously expending, for a limited period, if it is otherwise desirable to do so, the entire sum indicated as the maximum in the act." And the resolution they reported for the appropriation of $20,000 for a library was at that time adopted. Thus, in advocating the largest views in respect to the library that have ever been presented by a committee or sanctioned by the Board, —at the time and place when those, if such there were, who believed the law to require the Institution to be made "almost entirely a library" would surely put forth the whole strength of their cause,—no claim is made for it as the "paramount object," no right whatever is asserted to the maximum sum mentioned in the act; but the large appropriation they sought to devote to the library "for a limited period," is recommended solely on grounds of expediency; although motives found in an intimation gathered from the history of the act are introduced as sanctioning the propriety of their recommendation. We seek in vain for any intimation that they were *bound* to propose so large a sum as they did, or that they would have done so had they believed a smaller appropriation more expedient at the time.

The next action of the Board of Regents, a month later, when the committee raised "to digest a plan to carry out the provisions of the act to establish the Smithsonian Institution," had made its report, brings us to the passage of the now famous "compromise resolutions," as they are called. In these it was agreed that, after the completion of the building, one half of the annual income, or $15,000, should be appropriated to the library, the museum, collections, &c.; and the other half to the preparation and publication of transactions, reports, and other publications of the Institution, to aiding and stimulating original researches in all

departments of knowledge, to the payment for lectures delivered at Washington, &c.; the Institution thus entering upon the career of active operations for the increase and diffusion of knowledge for which it is so distinguished. We know not by what majority these resolutions were adopted. Their legality, however, appears not to have been questioned. No member entered any protest upon the record, as he surely would have done if he held them to be an infringement of the law. That the minority raised no question of the illegality of this action may be clearly inferred from the terms of the resolution which they supported as a substitute. This resolution, for which we are indebted to Mr. Meacham's Dissenting Report (Doc. 20, 5), is as follows:

"*Resolved*, That it is the intention of the act of Congress establishing the Institution, and in accordance with the design of Mr. Smithson, as expressed in his will, that one of the principal modes of executing the act and the trust, is the accumulation of collections of specimens and objects of natural history and of elegant art, and the gradual formation of a library of valuable works pertaining to all departments of human knowledge, to the end that a copious storehouse of materials of science, literature and art, may be provided, which shall excite and diffuse the love of learning among men, and shall assist the original investigations and efforts of those who may devote themselves to the pursuit of any branch of knowledge."

It merely affirms that the library, the museum, and a gallery of elegant art constitute "one of the principal modes of executing the act and trust." It does not deny that the "active operations" may be *a legal*, or even a proper mode. That denial is more recent.

The experience of six years having rendered it doubtful whether this absolute division of the income into two equal parts, irrespective of circumstances as they occur, was practically the best mode that could be devised, in March, 1853, a special committee of seven was raised to consider this subject. This committee made its report on the 20th of May, 1854, by its chairman, Mr. Pearce, all the members concurring except Mr. Meacham, who had replaced Mr. Choate on the committee a few months previous. The report, (Doc. No. 4,) after a general view of the character, condition, and operations of the Institution, actual and prospective, submitted two resolves; the first, repealing the resolutions passed in January, 1847, and then about to come into effect, which required an equal division of the income between the active operations on the one hand and the museum and library on the other; the second requiring "That hereafter the annual appropriations shall be apportioned specifically among the different objects and operations of the Institution in such manner as may, in the judgment of the Regents, be necessary and proper for each, according to its intrinsic importance, and in compliance in

good faith with the law." A restriction which the Regents had imposed on themselves, antecedently to experience, is here proposed to be removed. As, in the exercise of their discretion, they had adopted a fixed and unvarying apportionment, they now proposed, instead, to exercise the same powers and the same discretion (neither more nor less) from year to year, according to their best judgment at the time, and in compliance in good faith with the law.

The consideration of these resolutions was postponed until the next annual session of the Board, in January of the present year; and it was ordered "that said report, and such report of a minority of the committee as may be made in the recess of the Board, be printed." The committee's report was so printed, for the use of the Board; and, belonging to the proceedings of the Board, it properly made a part of the Eighth Annual Report of the Regents to Congress, which was closed on the 22d of July, presented on the 1st of August; and ordered to be printed both by the Senate and the House of Representatives. Up to this time, an interval of two months having elapsed, no minority report had been sent in, so far as we can learn, and none could therefore properly be embodied in the Annual Report, "showing the proceedings of the Board up to July 8, 1854." A minority report of the Hon. J. Meacham, (Doc. No. 5,) without date, and consisting of an elaborate review and criticism of the report of the Special Committee, is however appended to the House edition of the Smithsonian Report,—whether properly or not we need not inquire.* In this document it is for the first time officially maintained (though it had been anonymously argued in the newspapers some time before, and apparently broached in the sessions of the Board as early as the spring of that year): 1, That the "compromise resolutions" of Jan. 16, 1847, *were unauthorized by the law; and yet ought to be continued in force:* and 2, That their *repeal by the passage of the resolves of the Special Commit-*

* Not that any one could wisely object that such an official document as this is, or would become when laid before the Board, should have the widest publicity. But, as it happens to be printed in the House edition of the Smithsonian Report, and not in that of the Senate, which was somewhat earlier published, it formed the occasion of a charge made by certain newspapers, that the Secretary of the Smithsonian Institution *suppressed* in the Senate edition Mr. Meacham's report adverse to his views. Whereas, we find, 1, That no minority Report *had been made*, and therefore none could form part of the annual communication to Congress: 2, That the manuscript was not sent in to the Secretary at any time during the recess, so that the Secretary could cause it to be printed for the use of the Board, as was ordered: 3, It does not appear to have been furnished to the Senate unofficially (officially it plainly could not have been), and so could not possibly have been suppressed there: 4, Having been unauthorizedly annexed to the Mss. Smithsonian Report for the past year communicated to the House of Representatives, while in the custody of its Committee on Printing, and the question being raised in this Committee, the Secretary, on being consulted, urged no objection to its being printed, with the simple statement of fact prefixed "that it does not form a part of the proceedings of the Board of Regents reported to Congress."

tee would be a further illegality, unless accompanied by a resolve, that a compliance in good faith with the letter and the spirit of the charter requires that a large proportion of the income should be appropriated to the library.

Let it be noted, in passing, that the argument of Mr. Meacham's report, and this resolve appended to it, do not present the very same issue,—unless we interpret the latter by the former, which, so far as respects the writer's meaning and intention, we may properly do. The proposition, "that a compliance in good faith with the letter and spirit of the charter of the Smithsonian Institute requires that a large proportion of the income of the Institution should be appropriated for a gradual formation of a library," is one thing; but is not identical with the proposition, so elaborately argued in the body of the report, that the enactment in the 8th section of the law, directing "an appropriation, not exceeding an average of $25,000 annually, for the gradual formation of a library, composed of valuable works pertaining to all departments of human knowledge," legally and morally enjoins the Board to expend this maximum sum, or something like it, on the library,—requires them to expend sums which in a term of years shall amount to an average of $25,000 *per annum*, whether convinced that this is the best use of the income, or not.

That an influential portion of the Board, especially during its earlier years, felt that the library was the safest, and ought to be the most prominent, object of their fostering care, there is no kind of doubt. Some of these, on further experience, have changed their minds. One of them is Mr. Pearce, the author of the Special Committee's report, who now so ably advocates the "plan of active operations;" although, as Mr. Meacham is careful to inform us, he strenuously supported "the library plan" in the discussion upon the bill before the Senate;—which clearly shows that he did not understand the law to prescribe 'the library plan,' but only to permit it. Another portion (of which Mr. Douglas may be taken as a representative), looking to the history as well as to the words of the act, hold that Congress "did sanction the policy of a library as a principal but not an exclusive feature in the Institution,"—"that the law contemplates the library as a prominent object in the Institution, and that at least a majority of the funds should be expended in the building up of the library;"—the intention of the legislators, even when not prescribed in restricting terms, being held to limit, or at least to influence to a great degree, the discretion of the administrators. Such persons would properly adopt the resolves appended to Mr. Meacham's report, while they would repudiate his argument.

The doctrine of that argument appears to be novel. Perhaps it pre-existed in a latent state, awaiting the heat of popular controversy to germinate it at a favorable moment: but we have al-

ready shown that no trace of it is found in the published proceedings of the Board until about a year ago. The whole course of action has ignored it; and no Regent has entered a protest against that course as illegal.

We are at a loss to know whether Mr. Choate maintains this view of the law or not. His letter of resignation (Doc. No. 7), the only record of his views that has been published, however, implies that he does. He declares that Congress matured a plan, and sketched its general features with great clearness and completeness, in the law, the substantial meaning of which is palpable and unequivocal in its terms, and that this plan has been violated, and the law in effect repealed, by the action of the Board of Regents. Yet he dates the violation, not in January, 1847, when the compromise resolutions were passed, "according to which [only] a full half of the annual income was to be eventually applied in permanence to what I deem the essential parts of the plan of Congress," but in January, 1855, when these resolutions were rescinded, and the annual appropriations required to be made specifically for each object, "according to its intrinsic importance and in compliance in good faith with the law." It is hard to say how "henceforward the discretion of the Regents, and not the act of Congress, is to be the rule of appropriation" any more after than before this change; since that discretion which Mr. Choate *allowed* had permanently cut off one half of the income from what he "deems the essential parts of the plan of Congress," while that which he *denounces* renders practicable the appropriation of the whole available income to what he contends Congress meant it should be applied. Surely it is not the discretionary power of the Regents that is here complained of, but the direction in which they were about to exercise it. But what an anomalous discretion that must be which can be exercised only in one direction!

These newly developed claims of "the library plan," however based, it is understood were eloquently argued by Messrs. Choate and Meacham before the full Board of Regents, at the annual session in January last, the Chief Justice in the chair; and the decision was strongly adverse to them. Mr. Pearce's resolutions, rescinding the so-called compromise, and requiring specific annual appropriations, were carried by votes, the first of 8 to 6; the second of 9 to 5. This was not the test question; for they might with propriety have been opposed, and indeed were opposed, by Regents who do not regard the library as the paramount interest of the Institution; while those who do so regard the library,—not on grounds of general expediency, but on the ground that the law enjoins it,—would seemingly have been constrained to vote in the affirmative. The actual vote here is very important, and we must think decisive as to the true interpretation of the law, since

the same view is implicitly maintained by both parties. The majority voted to rescind the compromise resolutions, because they had no doubt of the right, and thought it most expedient, to do so. On the other hand, Messrs. Choate and Meacham's vote to continue them surely implies that they thought them lawful! Nor were they shut up to a choice between two evils, as will be seen by the proposition which they proceeded to offer. At any rate, their vote for the continuance of a rule of action which devoted only half the income to the library, museum, and gallery altogether, is a complete answer to their argument, or at least to the argument of Mr. Meacham's report. We dismiss, therefore, without further words, a doctrine which even its originators and supporters will not stand to.

The nicer questions remain: Though the act, interpreted by itself, does not enjoin such a large average appropriation for the library, may it do so, however, in virtue either of the intention of the testator, whose will is incorporated into the law *as its intent* (as Mr. Upham justly states), or of the intentions of any of the supporters of the act in Congress, to be gathered from recollection or from the printed debates? Or, can such intentions, avowed by the proposers of a clause of the act on the floor of Congress, but not effectuated in its terms, be held,—if not exactly to modify the obvious meaning of those terms, or to necessitate a particular interpretation of them,—yet still to control somehow the exercise of the discretion which the clause confers on the Board of Regents, so as to make it the duty of this Board to carry out such intentions in a general way, by expending on the library, not indeed the whole $25,000 said to be intended, but a large though indefinite part of it? The affirmative of one or both of these propositions, expresses, we believe, the whole case of 'the library interest' in the most favorable form. The argument of Mr. Choate before the Board of Regents may be presumed to have set forth the full strength of the case. Immediately after the passage of Mr. Pearce's resolves, which repealed former restrictions and laid the whole subject open, the point of these propositions was brought directly before the Board, unincumbered by other issues, by Mr. Meacham, who moved the following:—

"*Resolved*, That a compliance in good faith with the letter and spirit of the charter of the Smithsonian Institute requires that a large proportion of the income of the Institution should be appropriated "for the gradual formation of a library composed of valuable works pertaining to all departments of human knowledge."

"The question being taken on this resolution, it was lost, as follows:

"Yeas—Messrs. Choate, Douglas, Meacham, Stuart—4.

"Nays—The Chancellor, Chief Justice Taney, Messrs. Bache, Berrien, English, Hawley, Mason, Rush, Pearce, and Totten—9."

So the question was decided against the claims of the library interest by the strong vote of 9 to 4;—the Board deliberately affirming (what no vote had ever called in question) that full power and responsibility for the apportionment of the income among the objects of the Institution is vested in itself, subject only to the terms of the law. This is a legal question, and an official decision on the construction of the law by the body responsible for its administration. A judicial decision could hardly involve a higher responsibility. Of the nine Regents who affirmed this construction, all but two, we believe, are members of the learned profession, whose business it is to construe law; most of them have borne the responsibilities of judicial functions; and at least three of them rank among the most eminent jurists of the country, viz, the venerable Chief Justice of the United States—a member of the Board *in virtue of his exalted judicial office*,—and the two ex-Attorney Generals, Messrs. Rush and Berrien. Opposed to them are Judge Douglas, Mr. Stuart of Michigan (probably a lawyer), Mr. Meacham (late a clergyman), and Mr. Choate, whose forensic reputation is certainly unsurpassed.

The responsibility and the legal and moral weight of this decision satisfy our minds, and will doubtless satisfy the public. Had its grounds been given (as they would have been in case of a like judicial decision), it would be great presumption in us to continue our critical analysis; and as it is, our remarks shall be brief.

1. As to the intentions of the founder of the trust, and of individual promoters of the Act of Congress creating the establishment to execute the trust: which of the two are to be regarded, or most regarded, in the Construction of the law? Of course, the former; because the declared purpose of the Act is to execute the founder's will: the latter are of no consequence except as ancillary to the former, and of no force except as they may be embodied in the law. The law can have no intentions that are not expressed in its provisions or preamble: and the expressed intention of this law is, to provide for the faithful execution of the will of Smithson.

It is plain enough that the law does not in terms provide that the establishment it creates shall be mainly a library. Does it intend this, however, in virtue of any expressed or implied intention of the founder? Is it even supposable that Smithson had a library in view? The bequest was made "to found at Washington, under the name of the Smithsonian Institution, an establishment for the increase and diffusion of knowledge among men." It has often been said, that any man who regarded a library as the obvious, or the most important means for the increase and diffusion of knowledge among men would have been sure, in such a case, to have written '*library*' in place of '*institution*,' or

'*establishment.*' But even this does not fully express the point. Had Smithson been a book-collector, and left the will he did, who would have allowed that a library was intended? Is it not, however, equally preposterous to place this construction on these words of a man who had no library at all; whose twenty-seven published memoirs, it has been truly said, might every one of them have been prepared, and probably were prepared, without consulting a library; who during his lifetime labored to increase knowledge solely by original researches in chemical and physical science; and who bequeathed his fortune so that it should continue to furnish the means of increasing and diffusing knowledge in the world after his removal from it? In our minds, the only question is, whether a diversion of this fund mainly toward a library be an allowable use of it; and whether, in the appropriating clause of the act which, by fixing so high a maximum, would legalise this use of it, Congress did not unduly stretch its prerogative over the trust. We rest, however, in the hope and expectation that this clause will continue to be construed by its terms, and by the intention of the founder,—cordially agreeing as we do with the whole Judiciary committee of the Senate, that "this section cannot, by any fair construction of its language, be deemed to imply that any appropriation to that [maximum] amount, or nearly so, was intended to be required. It is not a direction to the Regents to apply that sum, but a prohibition to apply more; and it leaves it to the regents to decide what amount within the sum limited can be advantageously applied to a library, having a due regard to the other objects enumerated in the law." And the Judiciary Committee proceed to say:

"Indeed, the eighth section would seem to be intended to prevent the absorption of the funds of the Institution in the purchase of books. And there would seem to be sound reason for giving it that construction; for such an application of the funds could hardly be regarded as a faithful execution of the trust; for the collection of an immense library at Washington would certainly not tend 'to increase or diffuse knowledge' in any other country, not even among the countrymen of the testator; very few even of the citizens of the United States would receive any benefit from it. *And if the money was to be so appropriated, it would have been far better to buy the books and place them at once in the Congress library.* They would be more acceptable to the public there, and it would have saved the expense of a costly building and the salaries of the officers; yet nobody would have listened to such a proposition, or consented that the United States should take to itself and for their own use the money which they accepted as a trust for 'the increase and diffusion of knowledge among men.'"

2. As to the intentions of members of Congress who promoted the passage of the bill into a law in its actual form:—let all that can be justly claimed for the library plan on this ground be con-

ceded. These claims are very elaborately presented in Mr. Meacham's minority report (Doc. No. 5), to which we refer our readers. They amount substantially to this—First: Specific plans of operation, either contained in the original bill, or moved in amendments,—some of them prescribing the same 'active operations' that the Institution has undertaken,—were voted out. Well and wisely done, no doubt. No one specific plan, no enumerated set of operations, to be fixed once for all, could obtain the vote of a clear majority. Where various conflicting schemes are advocated, it is not difficult to outvote any one. The question turns, upon what is put in its place. We gather from 'the parlimentary history' of the Act, that the real majority did not wish to anticipate experience, and fix details, but to found an Institution, subject to a few general requirements; to give it able and responsible managers; to devolve on them the duty of reconciling as far as they could the conflicting plans so vehemently urged by several different parties, and to apportion the income among the specified objects according to the dictates of their best judgment, enlightened more and more by experience. So say the Judiciary Committee of the Senate.

"It is very evident, by the law above referred to, that Congress did not deem it advisable to prescribe any definite and fixed plan, and deemed it more proper to confide that duty to a Board of Regents, carefully selected, indicating only in general terms the objects to which their attention was to be directed in executing the testator's intentions.

"Thus, by the fifth section, the Regents were required to cause a building to be erected of sufficient size, and with suitable rooms or halls, for the reception and arrangement, upon a liberal scale, of objects of natural history, including a geological and mineralogical cabinet, also a chemical laboratory, a library, a gallery of art, and the necessary lecture-rooms. It is evident that Congress intended by these provisions that the funds of the Institution should be applied to increase knowledge in all of the branches of science mentioned in this section—in objects of natural history, in geology, in mineralogy, in chemistry, in the arts,—and that lectures were to be delivered upon such topics as the Regents might deem useful in the execution of the trust. And publications by the Institution were undoubtedly necessary to diffuse generally the knowledge that might be obtained; for any increase of knowledge that might thus be acquired was not to be locked up in the Institution, or preserved only for the use of citizens of Washington, or persons who might visit the Institution. It was by the express terms of the trust, which the United States was pledged to execute, to be diffused among men. This could be done in no other way than by publications at the expense of the Institution. Nor has Congress prescribed the sums which shall be appropriated to these different objects. It is left to the discretion and judgment of the Regents."

And further,

"No fixed and immutable plan, prescribed by law or adopted by the Regents, would attain the objects of the trust. It was evidently the in-

tention of the donor that it should be carried into execution by an Institution, or Establishment, as it is termed in his will. Congress has created one, and given it ample powers, but directing its attention particularly to the objects enumerated in the law; and it is the duty of that Institution to avail itself of the lights of experience, and to change its plan of operations when they are convinced that a different one will better accomplish the objects of the trust. The Regents have done so, and wisely, for the reasons above stated."

But, secondly, Mr. Meacham contends that the appropriating clause, fixing the maximum expenditure on the library at $25,000, having been introduced into the bill with the intention, avowed by its advocates in Congress, of making the Institution "almost entirely a library," therefore, "it seems impossible to doubt that Congress in the charter of the Institution intended to indicate that the library . . . should receive about $25,000, a year, and that the museum, laboratory, and lectures should receive only abont $6000 a year." This argument, somehow, did not convince the Chief Justice and the rest of the lawyers; who probably reflected, —1, that the intention of these advocates might not be the intention of the majority who voted for the clause, and still less the intention of Congress; many may have preferred to admit the clause, although involving the *possibility* of the diversion of the bequest to a library, rather than by its rejection to hazard the passage of the otherwise satisfactory bill into which it was inserted:—2, that these advocates well knew how to express their intention in the terms of the clause, had they been so minded, having a pattern in the bill that passed one House the year before, in which $20,000 was *required* to be annually expended on the library, and that they would have so worded the present clause of the 8th section had they felt able to carry it;—and, 3, that, since it could no better be, they were content with the opportunity which Congress (we think unadvisedly) afforded them of accomplishing their end in the Board of Regents, whenever they could convince this body that a library was a proper, and the best practicable instrument for fulfilling the purpose of Smithson's bequest. This they have not yet succeeded in doing. We cannot believe that they did not well know the difference between the mandatory appropriating clause in the former bill, and the permissive one of the present act. But even if we suppose they did think that their intention was by some implication fixed upon the clause in question, is that intention to override or qualify its terms? An exactly parallel case would have arisen had the Board of Regents adopted (instead of rejecting, 9 to 4) Mr. Meacham's resolution appended to his report, and quoted above; viz, that a compliance with the law requires that *a large proportion* of the income should be appropriated for a library; the intent and meaning of which, in the mover's mind, as expressed in the body of the report, being

that this "large proportion" is an average of $25,000 a year. Would the Board have adopted the proposition with Mr. Meacham's understanding of it, or merely in its obvious terms? Who ever heard of a legislation which devolves a discretion over the amount of an appropriation upon a Board of trustees, but yet tacitly assumes that this discretion shall not be exercised at all; or shall be exercised only within some narrow and undefined limits, to be gathered, not from the act itself, but from the intentions and motives of a few of its promoters; which again are to be gathered from their speeches in Congress! And this on a point involving the construction of a will and testament, in which parole evidence is of no account.

Such are some of the considerations which render manifest to us the correctness of that construction of the law which the Regents have all along acted upon, and have lately (when impugned) formally affirmed. It has been re-affirmed and sanctioned, in the fullest terms, by the unanimous voice of the Judiciary Committee of the Senate (which is always chosen from among the ablest jurists of that body), when referred to them by a motion of the Hon. Mr. Clayton, made on the express ground that, "if the Regents are right in the interpretation they have given to the law, they should be sustained by the judgment of the Committee and the judgment of the Senate." It has been adopted in the report of one part of the special committee raised by the House; and it is scarcely attempted to be controverted by the other.

We have devoted our attention principally to the question of the *right* rather than the *expediency* of the actual policy of the Smithsonian Institution; because it is on this that the force of the charge falls; and because, for the want of a proper analysis of the case, some friends of this policy have been in doubt whether it is fully compatible with the terms and spirit of the law. That once settled, the propriety of the course pursued by the Institution need not be argued to the readers of this Journal. It is heartily approved by the whole scientific interest of this country; it appears to be favorably regarded by a large share of our best scholars and literary men. Its earnest endeavors to do that which cannot be done, or so well done, by existing institutions; to turn to utmost account the means of diffusing and increasing knowledge at its disposal; its refusal to stereotype one plan, and that the most localized, for all circumstances and times, and to give to place or "party what was meant for mankind," are most consonant to the spirit and presumed intentions of the founder, and are, we believe, regarded with high satisfaction by the learned world. Spontaneous and ample evidence of this comes day by day into our hands. We think the Regents do well, therefore,—instead of expending all or most of their means

upon a universal library at Washington (thus providing from a foreigner's bequest for a want which the General Government may be expected to supply, and are now supplying quite as largely as the whole income of Smithson's fund would),—to accumulate by degrees a "library of valuable works pertaining to all departments of human knowledge," with special reference to those most needed by the investigator and enquirer, and not otherwise accessible, and to journals and transactions of all learned societies, in which the principal advances in knowledge are recorded; to promote original researches in every department of enquiry; to publish and freely distribute important "contributions to knowledge," as well as reports giving an account of the discoveries in science from year to year wherever made; to organise and direct, and whenever necessary conduct, an extended and well-directed system of meteorological observations over the length and breadth of the country; to aid scientific explorations, especially such as tend to bring to light the natural resources and productions of America; to gather and arrange collections, especially of our own natural productions, and such as illustrate and verify the publications and researches made by the Institution, or under its auspices; to solve important experimental problems in chemistry, physics, &c., or to furnish instruments and facilities for so doing; and to continue the system of general international scientific and literary exchange, by which every institution of learning and science in the United States is enabled to carry on its correspondence, transmit its publications to all similar societies in any other part of the world, and to receive theirs in return, almost without expense,—an economising of the feeble and scattered scientific means of the country, which largely contributes to the diffusion and increase of knowledge. Some details respecting this good work of the Smithsonian Institution would be interesting and perhaps new to many of our readers. But our space is too limited to give them.

We cannot, therefore, attempt an analysis of the two reports which have emanated from the special committee raised in the House of Representatives, the latest documents on our list. Nor is this necessary; for they throw no new light on the question with which we have here been mainly occupied. We will barely notice, in passing, one or two statements in the report (Mr. Upham's) which, to a considerable extent, sustains views adverse to those of the Board of Regents. There are three new suggestions in this report, of whose value our readers can form their own opinion. Two of them look to alterations of the law, viz: that the executive affairs of the Institution should be administered by two officers of equal authority; and that the Institution itself might be made a bureau under a department of the General Government, instead of remaining as now, a trust administered by an independent establishment. The third propounds a novel con-

struction of the words of Smithson's will, viz, "the increase and diffusion of knowledge among men," which we give in full, to exemplify the straits those are reduced to who contend that the founder's design would really be attained by a library at Washington. Says Mr. Upham:

"The words 'among men' were used merely to corroborate the idea expressed by the word 'diffusion.' They do not necessarily imply that the Institution should confine itself to world-wide operations. The word is not, as some seem to suppose, 'mankind' but 'men;' and he diffuses knowledge 'among men' as truly, and in as full a sense, when he enlightens the minds of his neighbors, as of persons at the very farthest pole."

Really would not this construction be about as good as now, even if the word "mankind" had been used, instead of "men?" And although the words "do not necessarily imply that the Institution should *confine* itself to world-wide operations,"—an odd sort of confinement indeed,—do they not imply that it should *extend* itself "to world-wide operations?" Again:

"The word 'INCREASE' is held by some of the zealous combatants in the Smithsonian controversy to be identical with 'DISCOVERY.' The idea seems to be that knowledge can only be *increased* by the *discovery* of *new* truth. This is an arbitrary and untenable position. A mind experiences an increase of knowledge if it knows more than it did before, although all the ideas it has received may be in the commonest textbooks. There has been an increase of knowledge in the school, or the congregation, or the lecture-room, if ideas not before known to them have been received into the minds of the hearers, even; indeed it matters not if those ideas have been recorded for thousands of years in languages, classical or sacred, that have been dead long ago. Knowledge has been increased if one mind has received more, whether it be new or old truth. The language of Smithson is perfectly simple, and in its natural sense covers the whole ground; it includes but does not require *new* truth. Truth discovered a thousand years ago is as good as truth discovered yesterday. Knowledge embraces it all alike, and Smithson's object was to carry knowledge where it was not before, and to increase it where it was; to spread it over a wide area and to a greater depth."

We would only ask; if the communication of known facts and ideas from one to another constitutes "*the increase of knowledge*" in the sense of Smithson's will, then what did Smithson intend, and what does Mr. Upham mean, by "*the diffusion of knowledge?*"

To show the impropriety of publishing memoirs, like those in the Smithsonian Contributions to Knowledge, a fear is expressed that favoritism will be practiced, injury inflicted on some individuals through a condemnation of their treatises by "a secret tribunal," resentments enkindled, and perhaps important discoveries be suppressed. With more reason might the same complaint be

made against this Journal: for a greater amount of acceptable matter is offered us than we can publish, and we are obliged to make our own selection; while the Smithsonian Institution, in each case, calls in the aid of the ablest experts it can find in the country, and holds their published names responsible for their judgment. And the author of a rejected memoir could surely have his treatise referred to a second committee, or bring it before the world in some appropriate periodical journal, or give it to some one of our learned societies; which are willing enough, and generally able, to publish any such really important contribution to knowledge, if not accompanied by numerous and costly plates, and always to publish as much as is requisite to bring the new discovery or new ideas before the world. The publication of full details and of the needful pictorial illustrations is indeed important; and herein the Smithsonian Institution is of great use, by publishing valuable memoirs with illustrations which are beyond the means of the author and of our learned societies: but this service is rendered, not so much to the author, who may always bring out the substance of his views so as to secure the merit of discovery, as to the public he addresses, who are thereby furnished with the means of fully comprehending the case, and drawing their own conclusions from his data. Mr. Meacham, adopting an opposite view, expresses the opinion that, without the agency of the Institution, the present Smithsonian memoirs "would all have been published, some in journals or transactions of societies, and some in separate books,—not, perhaps, in so elegant a style." This, we know, is not the case as respects some of the larger memoirs; and if the rest had been so published, an equal quantity which the principal learned societies and journals have in the meantime given to the world could not have appeared.

The suggestion that the Institution might be administered as a bureau of the Department of the Interior, strikes us as by no means happy, nor compatible with the nature of the trust,—a private foundation for the public benefit, to be administered not by the executive government, but by an establishment for the purpose. We think that neither this, nor Mr. Upham's extraordinary suggestion, that the Institution "would be found to work most auspiciously" under two coördinate executive officers, is likely to be seriously entertained. Nor can we agree with him that the present system is faulty, and "necessarily surrounded with very great difficulties." The whole committee bear witness to the "zeal, sincerity, integrity, and high motives and aims" of "those who suggested and have labored to carry out that system." Let such administrators have opportunity to give that system a fair trial, free from the embarrassments of "an antagonistic position" on the part of any one charged with the duty of car-

rying their views into effect, before any actual or anticipated practical difficulties are charged upon the present organization.

It does not fall in our way to discuss the question, whether the Secretary possesses the right to dismiss, for assigned reasons, a subordinate for whose official conduct he is responsible to the Board. Let it suffice that the Board of Regents had formally declared this power to belong, in their opinion, to the Secretary (the Chief Justice, then absent, spontaneously adding his concurrence), and this in direct reference to its application in the only case of its exercise that could admit of a doubt. We would not question the motives of the late Assistant Secretary in charge of the library, nor lay down our rule of conduct for others. But we ourselves should not think it becoming and proper to continue to hold and receive the emoluments of a responsible appointment, under a Secretary against whom we had made charges, and after those charges had been pronounced unfounded by the Board, nor while our sense of duty to the public impelled us to labor for the subversion of the recognised policy of our employers, and to defeat the re-election of an *ex-officio* member of the Board; and we should think it passing strange to be allowed to do so. The statements of the two reports from the House Committee equally and effectually dispose of the case of Mr. Blodgett, which has also been paraded as a grievance in the public prints.

The testimony quoted, in the last paragraph but one, from Mr. Upham's report, is a sufficient answer to the allegation and insinuation of unworthy motives with which most of the articles in newspapers and magazines, assailing the Regents and the Secretary of the Institution, have abounded. In one notable instance, a writer in a popular magazine does not spare even the memory of the noble founder, but calls him a "poor, proud, insignificant dabster in the chemistry of tears, and the mathematics of *rouge et noir*," and scouts the idea of paying any regard to his will in the construction of the law for executing the bequest he entrusted to our country's guardianship.

The attempt to form a spurious public opinion by appeals to men's prejudices, and by the iteration of perverted statements cannot be long successful. Public opinion may have been misled in some quarters for a season; but juster views are sure to prevail,—we think are generally prevailing already.

NOTE.—Just as this sheet is going to press we have received a copy of the official edition, just issued, of the two Reports from the House of Representatives' Select Committee (Mr. Upham's and Messrs. Witte and Taylor's), with various documents annexed. A hasty perusal of these documents shows nothing requiring any modification of the statements and argument of the present article; although there is much that might call for remark. We are informed on good authority that a part of these papers had never been before the committee, **but must have** been appended after its latest sessions, without the knowledge or consent of a majority of its members; while some documents that were before the committee are omitted! [May 10th.]

ART. II.—*Description of a new species of Clathropteris, discovered in the Connecticut Valley Sandstone;* by E. HITCHCOCK, Jr., M.D.

THE position of the sandstone of the Connecticut valley is of no small interest and importance to the Geologist, and has never been positively determined. This is mainly owing to the fact, that with the exception of the footmarks, very few well characterised fossils have been discovered in this formation.

During the last summer a specimen of a fossil fern was found in the sandstone of Mt. Tom, in Easthampton, Mass., which seems without doubt to belong to the genus *Clathropteris* of Brongniart. Previous to this discovery several specimens of this genus had been found, and are now in the Cabinet of Amherst College; but none so well defined as to indicate the genus to which they now undoubtedly seem to belong.

The generic description of this plant is thus given by Brongniart: "Folia pinnatifida, pinnulis elongatis, nervo medio valido percursis: nervis secundariis pinnatis parallelis simplicibus, nervis transversalibus unitis, areolas subquadrangulares efformantibus, nervulis tenuoribus reticulatis notatas," who considers the quadrangular spaces formed by the veins and veinlets as the decisive characteristic.

Only one species is as yet described, "meniscioides," named from the resemblance of the spaces between the veins, to a meniscus.

To the East Hampton species we have provisionally attached the name *Clathropteris rectiusculus*, from the supposed absence of the meniscoid character. We offer the following description:—

Clathropteris foliis profunde pinnatifidis, pinnulis rachi adnatis, inter se discretis, oblongis, bipedalibus vel majoribus; nervis secundariis rachi subperpendicularibus, 4 lineis distantibus; nervis transversalibus rectiusculis vel leviter arcuatis clathratis, nervulis tenuissime reticulatis, superficie foliorum planiuscula.

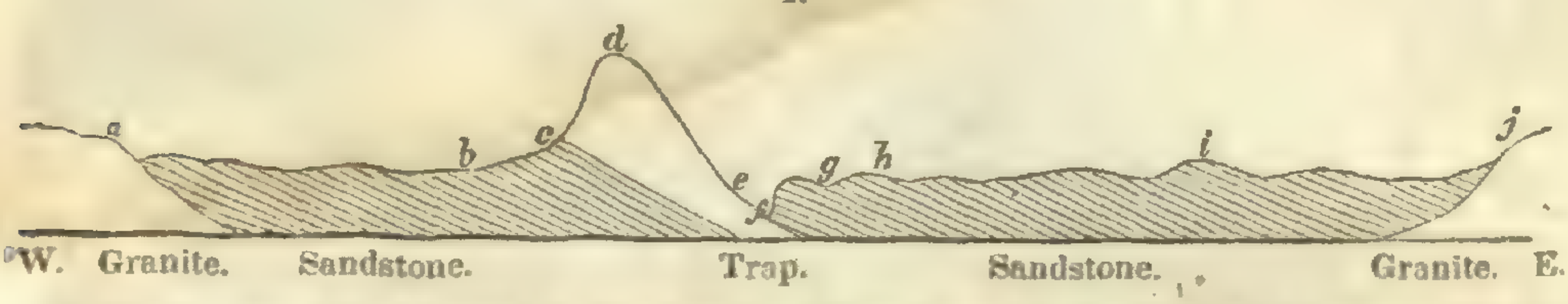
Geological position of the European Species.—In 1828, Adolph Bronguiart regarded this fern as very characteristic of the Lias sandstone, which (fern) had been discovered in these localities, and he thus speaks concerning it: "This fossil fern, so remarkable in its structure, and by its well marked relations to a small group of living ferns, is not less so in a geological point of view. In reality since I have found it at Hoer in the arkose formation, all the fossil plants of which compel me to refer it to a position between the lias and the chalk, this same plant has been found in two other localities where the geological position is well determined; at Mt. St. Etienne near La Marche in the Vosges, in a

formation which Elie De Beaumont considers as certainly belonging to the lias; and at Pouilly in Auxois, by Messrs. Bonnard and Beaumont in a bed of arkose of the same epoch of formation; both of which go to prove that this plant is one of the most important characteristics of the lias, and it is probable that it will appear again in more localities where this formation exhibits itself."*

Nine years later, Prof. Bronn, in his Lethæa Geognostica, modifies these statements of Brongniart as follows; "as yet no determinate position has been found for the sandstone of Hoer in Scania, which Adolph Brongniart includes in his common group of keuper, variegated marl, and lias, and Clathropteris meniscioides appears therein as well as in the Lias sandstone of La Marche in the Vosges, along with Equisetum columnare, which latter sandstone formation however (that of the Vosges) as it appears belongs to the keuper, in which Alberti again notices this fern in Neuen Welt near Basle."†

In 1849, in his Index Palæontologicus, Bronn places the Clathropteris both in the Lias and the Keuper.‡

1.



a, Southampton lead mine; *b*, Easthampton Seminary; *c*, locality of Clathropteris; *d*, Mt. Tom; *e*, locality of foot-marks; *f*, Connecticut river; *g*. Mt. Holyoke Seminary; *h*, Hadley; *i*, Granby; *j*, Rock Rimmon.

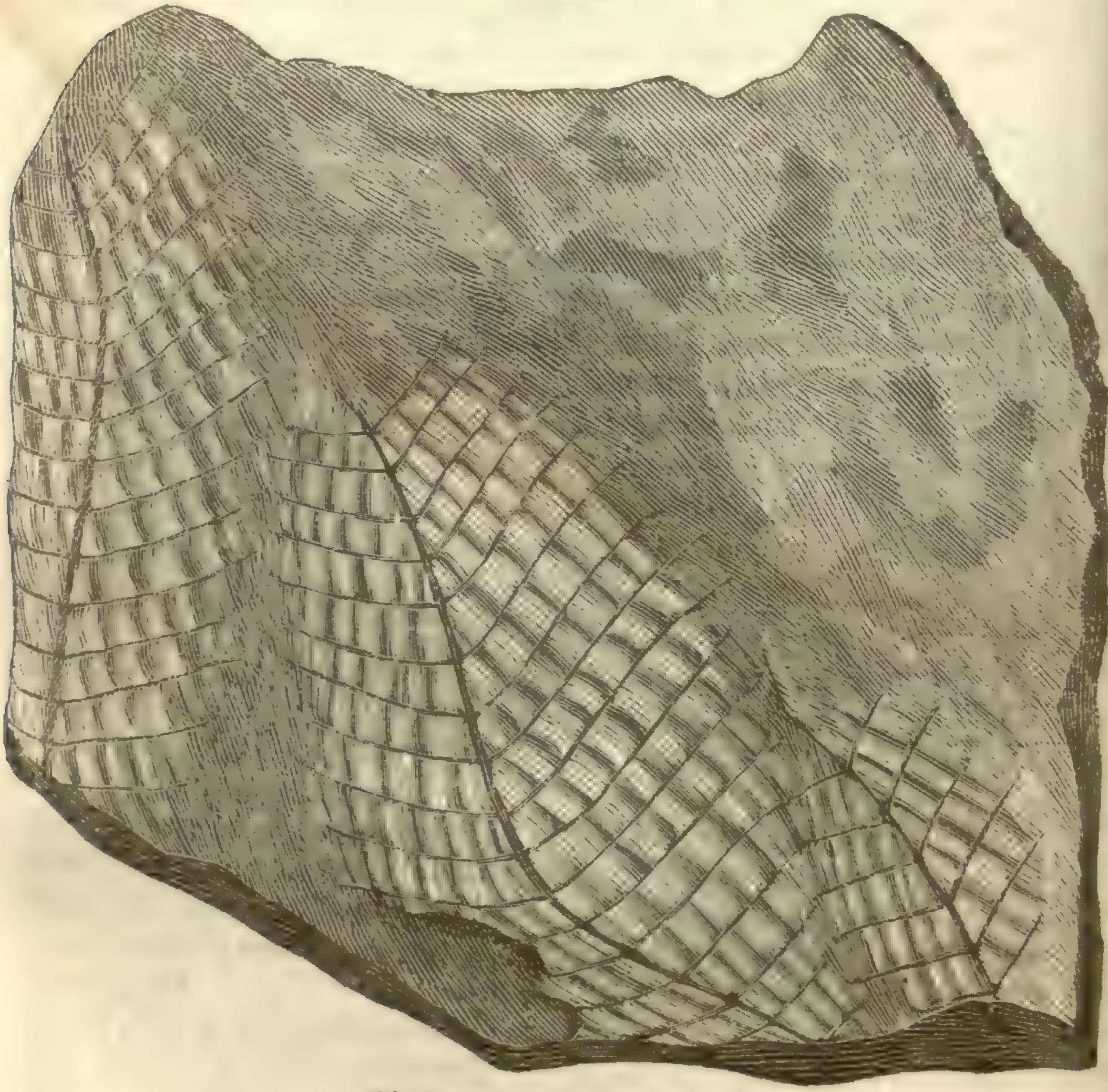
The Clathropteris of East Hampton is found in a coarse reddish sandstone, on the west face of Mt. Tom. The upper part of this mountain is trap, beneath which the sandstone crops out with an easterly dip of about 25°. The sandstone has a southeasterly dip across the whole of the Connecticut valley. That east of the trap range is made up of finer materials, and is of a more slaty character than that on the west. Bassett's Quarry, where this fern occurs is somewhat west of the middle of the valley, as may be seen in the annexed section. In this section lately measured by my father—Dr. Hitchcock of Amherst College—it is found that the thickness of the sandstone east of Mt. Tom is more than 8,000 feet, and that on the west, or below Mt. Tom is nearly 5,000 feet. Supposing one half of this thickness to be accounted for by original deposition on an inclined surface, there will still remain a thickness of some thousands of feet both above and below the locality of the fern. A thickness equally great for the sandstone of the Connecticut valley in measuring another sec-

* Brongn. Veg. Foss., p. 131 and 132.
† Lethæa Geognostica, vol. i, p. 140.
‡ Index Palæont., vol. ii, p. 22.

tion across the valley at Turner's Falls, 30 miles north of East Hampton.

Fig. 2 exhibits portions of several fronds or perhaps only pinnules of the East Hampton fern. Some of these pinnules—although much broken at their extremities—are nearly a foot long, and 4 inches wide, so that probably they were originally at least one foot and a half in length.

2.



Clathropteris rectiusculus.—H.

The pinnule on the left hand side of the figure, shows the delicate secondary veins, which are usually obscure and therefore this fern has not heretofore been recognised. I have presented to the Cabinet of Amherst College, a large specimen from E. Hampton containing numerous isolated fronds, and in one place showing a large number most distinctly radiating from a center, like the tree ferns of tropical regions. These radiating fronds are broken off at their extremities, being only 4 to 6 inches in length. The specimen which is figured measures nine by thirteen inches, and is in the Cabinet of the Williston Seminary at East Hampton.

In the Cabinet of Amherst College is a fine specimen of a radiating Clathropteris (that is its apex) from the quarry of Roswell Field in Gill, Mass., and although nearly one quarter of the circle is lost, yet as many as 17 distinct fronds can be counted radiating from one stem. Although the reticulated character of this specimen is rather obvious, it was not noticed till the more recent specimen from East Hampton was discovered.*

In the same cabinet is another obscure specimen of the Clathropteris from the banks of Connecticut river in Montague, two miles southwest of Field's quarry, a sketch of which is given in my father's final report on the Geology of Massachusetts, vol. ii, p. 452.

The specimens from Gill and Montague occupy nearly the same position in the sandstone as the one at E. Hampton, although they are on the east side of the trap range.

Conclusions.—The above facts make it almost certain that a species of Clathropteris occurs in the sandstone of the Connecticut valley not far from its center, measuring across the strata, and near to the interstratified beds of trap both above and below. Now since this fern is found in Europe only in the upper part of the Trias, and the lower part of the Lias, it is very probable that it occupies the same geological position here. If so we ascertain the existence of a zone of rock in the Connecticut valley not far from the junction of the Lias and the Trias. And since two measurements of sections across this valley show a thickness of sandstone strata both above and below this zone thicker than the Lias and the Trias of Europe, the probability seems very strong, that the equivalents of both of these rocks exist here, and not improbably some others both older and newer.

If we can rely with confidence upon this geological zone, it will form a convenient starting place for tracing out other older and newer formations.

It will be seen that while the above conclusions sustain the opinion lately advocated with much ability by Prof. W. B. Rogers (Am. Jour. Sci. Jan., 1855, p. 123), that the Lias sandstone exists in the valley of the Connecticut, it makes the opinion also probable—long since advanced by my father that the Trias also exists here.

The radiations in the specimen above noticed appear to be distinct and entire fronds, and so resemble those on fig. 2, as to make it probable that the latter also are fronds and not pinnules.

* I have reason to suppose that this specimen has been mistaken for a Zamia.

ART. III.—*On the Periodical Variations of the Declination and Directive Force of the Magnetic Needle;* by W. A. NORTON, Professor of Civil Engineering in Yale College.

(Continued from vol. xix, page 211.)

IN the calculations made on pp. 207, 208, of the relative effects of the ecliptic and radial currents, no account was taken of the possible effects of *residual* currents, that is, of the gradually subsiding currents which may succeed those which are directly developed by the sun's action. I now propose to inquire into the nature and extent of their action, to make due allowance for it, and to apply additional numerical tests to the theory under examination. But I would previously remark that there is no occasion to distinguish the action of the primary from that of the secondary ecliptic currents, so called, at the equinoxes, in considering the entire effect in the interval from 6 A. M. to noon or from noon to 6 P. M. (See p. 205.) It is only when we are comparing the hourly variations that this distinction need be made. In addition to this qualification of certain statements made on p. 205, it should be observed (although the correction is of little consequence) that the secondary current, previous to 6 A. M., is in fact equally inclined to the meridian at the two equinoxes.

If we confine our attention to the entire semidiurnal interval just mentioned then the ecliptic and radial currents tend, both in the forenoon and afternoon, to deflect the needle in the same direction, at the autumnal equinox, but in opposite directions at the vernal equinox. Disregarding, for the present, the residual currents, let r denote the semidiurnal effect of the radial and e that of the ecliptic currents at the equinoxes; then taking the data from Table I, (p. 194,) we have, for the forenoon, $r+e=8'\cdot65$, $r-e=5'\cdot67$, and therefore $r=7'\cdot16$ and $e=1'\cdot49$. For the afternoon, $r+e=7'\cdot89$, $r-e=2'\cdot78$; hence $r=5'\cdot33$, and $e=2'\cdot55$. The two values of r differ from each other because of the action of residual currents. As a first approximation to the determination of the semidiurnal effect of the residual radial currents, let R = diff. of effects of the primary radial currents set in circulation at the beginning and end, respectively, of either the forenoon or the afternoon interval, and x = effect of the residue of all the radial currents developed during either interval; then $R+x=7'\cdot16$, $R-x=5'\cdot33$, $R=6'\cdot24$, $x=0'\cdot92$. Making the same calculations with the data from Table II, (p. 208,) we have, for the forenoon, $r=7'\cdot59$, $e=1'\cdot91$; for the afternoon, $r=4'\cdot50$, $e=2'\cdot03$: also $R=6'\cdot04$, $x=1'\cdot54$. $e=\frac{2'\,03+1'\cdot91}{2}=1'\cdot97$. The average of the two values of e found above, from Table I, is $2'\cdot02$. The difference of the two determinations is only $0'\cdot05$.

At the summer solstice, we have, for the forenoon $R+(e+x)=9'\cdot69$, for the afternoon, $R-(e+x)=3'\cdot80$; whence $R=6'\cdot74$, and $e+x=2'\cdot94$. The value found for R at the equinoxes is $6'\cdot24$, and of x, $0'\cdot92$; hence to find x at the summer solstice, we have the proportion $6'\cdot24:6'\cdot74::0'\cdot92:x=0'\cdot99$. Thus $e=2'\cdot94-x=1'\cdot95$. If we make use of Table II, we obtain $R=7'\cdot01$, $x=1'\cdot79$, $e=2'\cdot95-x=1'\,16$. At the winter solstice, in the forenoon, $R+x-e=1'\cdot15$, in the afternoon $R-x+e=1'\cdot62$; whence $R=1'\cdot38$, $e-x=0'\cdot23$. To find x, $6'\cdot24:1'\cdot4::0'\cdot92:x=0'\cdot21$. $e=0'\cdot23+x=0'\cdot44$. Table II gives $R=2'\cdot56$, $x=0'\cdot65$, $e=x-0'\cdot23=0'\cdot42$. It is to be observed that at the two solstices the ecliptic currents cross the meridian at right angles, at the hour of noon, and that therefore the values of e just determined show the effects of the ecliptic currents on the declination at 6 A. M. or at 6 P. M. At the equinoxes these currents have but little if any sensible effect on the declination at 6 A. M. and 6 P. M., but cross the meridian obliquely at noon, and therefore the value of e at the equinoxes shows the effect of the ecliptic currents on the declination at noon.

Having obtained these first approximations let us now proceed to a more minute investigation ;—taking into account the residual ecliptic as well as radial currents.

Let R= deflection, in the semidiurnal interval, that would result from the variation in the intensity of the radial currents.

E= effect, during same interval, of the variation of the ecliptic currents.

r= effect at noon of the residue of the radial currents developed during the forenoon.

e= effect at noon of the residue of the ecliptic currents developed during the forenoon.

r'= effect at 6 P. M. of the residue of the radial currents developed during the afternoon.

e'= effect at 6 P. M. of the residue of the ecliptic currents developed during the afternoon.

mr= portion of residual radial currents at noon that pass off in the afternoon.

ne= portion of residual ecliptic currents at noon that pass off in the afternoon.

$r=r'$ (nearly) ; $e=e'$ (nearly).

At the Equinoxes; in the forenoon, we have (from Table I); Aut. Eq., $R+r+E-e=8'\cdot65$; Ver. Eq., $R+r-E-e=5'\cdot67$. Hence $E=1'\cdot49$, $R=7'\cdot16+e-r$. $r>e$, whence $R<7'\cdot16$. For the afternoon the equations are, Aut. Eq., $R+E+mr-r'-ne-e'=7'\cdot89$; Ver. Eq., $R-E+mr-r'-ne-e'=2'\cdot78$. From which we obtain $E=2'\cdot55$, $R=5'\cdot33-(mr-r'-ne-e')=5'\cdot33+r(1-m)+e(1+n)$, (nearly). No account has been taken of the residual

action at 6 A. M. of the nocturnal currents, but this must be very slight, especially as the effect of the ecliptic currents is reduced to zero at that hour. Since this action must be the same at one equinox as at the other it can have no effect on the value of E. Its only effect will be to increase slightly the value of R. For the determination of r, we have the equations $R=5'\cdot33+r(1-m)+e(1+n)\ldots(1)$, $R+r=7'\cdot16+e\ldots(2)$; whence $r=\frac{1'\cdot83-ne}{2-m}$ (3).

At the *Summer Solstice;* for the forenoon, $R+E+r-e=9'\cdot69$ (4), for the afternoon, $R-E+mr-r-ne-e=3'\cdot80\ldots(5)$. Whence $E=2'\cdot94-\frac{r(2-m)+ne}{2}\ldots(6)$, $R=6'\cdot74+\frac{e(2+n)-mr}{2}$ (7). To take account of the residual action of the nocturnal currents, at 6 A. M., let x denote the effect of the residual radial currents at that hour, and y that of the residual ecliptic currents: x and y will each tend to diminish the declination (West), and therefore, supposing these currents to pass off entirely during the forenoon, instead of equation (4) we have $R+E+r-e+x+y=9'\cdot69\ldots(8)$, and instead of (6) $E+y=2'\cdot94-\frac{r(2-m)+ne}{2}+\frac{y-x}{2}$ (9); also $R=6'\cdot74+\frac{e(2+n)-mr}{2}-\frac{y+x}{2}\ldots(10)$. At the Winter Solstice, $R-E+r-e=1'\cdot15\ldots(11)$, $R+E+mr-r-ne-e=1'\cdot62\ldots(12)$; and thus $E=0'\cdot23+\frac{r(2-m)+ne}{2}\ldots(13)$, and $R=1'\cdot38+\frac{e(2+n)-mr}{2}\ldots(14)$. If we take account of the residual nocturnal currents in action at 6 A. M., $E+y=0'\cdot23+\frac{r(2-m)+ne}{2}+\frac{y-x}{2}\ldots(15)$, $R=1'\cdot38+\frac{e(2+n)-mr}{2}+\frac{x+y}{2}$ (16).

If we add together eqs. (6) and (13), accenting the letters of eq. (13), we obtain $S=E+E'=3'\cdot17-\frac{(2-m)(r-r')+n(e-e')}{2}$ (17). Since E and E′ represent the effects of the ecliptic currents on the declination at the hour of 6 A. M. at the summer and winter solstice, respectively, this formula should give the deflection of the needle at that hour, due to the ecliptic currents in the interval between the solstices. To take account of the residual nocturnal currents we must introduce into the formula the terms $\frac{y+y'}{2}-\frac{x+x'}{2}$. The exact value of this expression cannot readily be found, but we know that it must be a very small quantity. In the calculations which follow it is neglected.

The determination of the deflection produced by the ecliptic currents, in the interval between the solstices, from formula (17) admits of verification. We have seen (p. 201), that, for the hour of 6 A. M., the entire deflection in this interval is the sum of the effects of the ecliptic and radial currents, and that for the hour of 6 P. M. the deflection during the same interval is the difference of the effects of these two systems of currents. Now let

E= effect of the ecliptic currents at 6 A. M. or 6 P. M.; R= effect of radial currents at the same hours; e and r the effects of the residual ecliptic and radial currents, respectively, at 6 A. M.; and e', r', the effects of the two systems of residual currents which are in action at 6 P. M. Then, (by Table I,) $E+R+e+r=4'\cdot90$,

$$E-R+e'+r'=1'\cdot46\text{ ; whence, }E+e=3'\cdot18-\frac{e'-e}{2}-\frac{r+r'}{2}\quad\ldots(18).$$

It will be seen, when we come to discuss the variations of the horizontal force, that the intensity of the ecliptic currents is twice as great, at the hour of noon, at the winter as at the summer solstice; but on the other hand these currents pass off, from the vicinity of the zenith, about $4\frac{1}{2}$ P. M. It is probable, therefore, that the effect of the residual ecliptic currents at 6 P. M. is nearly the same at the two solstices, and accordingly that e', which is the difference between the individual effects at the solstices, is a small quantity in comparison with r'. $\frac{e}{2}$ and $\frac{r}{2}$ are also small quantities. If we neglect $\frac{e'-e}{2}$ and $\frac{r}{2}$, we have D=

$$E+e=3'\cdot18-\frac{r'}{2}\text{ (nearly)}\ldots(19).$$

This formula should give nearly the same value for the deflection as formula (17).

The following Table contains the results given by the formulæ which have just been investigated, corresponding to certain assumed values of m and n.

	Equinoxes.				Summer Solstice.				Winter Solstice.				S	D
	R	E	r	e	R	E	r	e	R	E	r	e		
$m=1$, $n=1$	6′·24	2′·02	1′·37	0′·46	6′·40	2′·14	1′·37	0′·23	1′·57	0′·50	0′·31	0′·23	2′·64	2′·65
$m=0$, $n=0$	6·55	2·02	0·92	0·31	6·89	2·02	0·92	0·15	1·53	0·43	0·20	0·15	2·45	2·82
$m=\frac{1}{2}$, $n=\frac{1}{2}$	6·43	2·02	1·10	0·37	6·69	2·07	1·10	0·18	1·54	0·46	0·25	0·18	2·53	2·76
$m=\frac{3}{4}$, $n=\frac{3}{4}$	6·35	2·02	1·22	0·41	6·56	2·10	1·22	0·20	1·55	0·47	0·27	0·20	2·58	2·70

The process of calculation is as follows: It was first assumed that at the equinoxes the semidiurnal effects of the residual radial and ecliptic currents, (r and e), bear the same ratio to each other as the corresponding direct effects of the two sets of currents, (R and E), and as we have seen (p. 26) that at the equinoxes R=3E (nearly), we have also $r=3e$. Introducing this value of r into equ. (3), we obtain the value of e, and thence the value of r. R has nearly the same value at the summer solstice as at the

equinoxes; the same is therefore true of r. It will be seen in the sequel that the semidiurnal effect of the ecliptic currents, on the horizontal force, is twice as great at the equinoxes as at the summer solstice; from which it may be inferred that the actual intensity of these currents, and therefore also the effect of the residual ecliptic currents, on the declination, (e) is twice as great at the one epoch as at the other. In the discussion of the variations of the horizontal force it will also appear that the intensity of the ecliptic currents is nearly the same at the winter solstice as at the equinoxes, but as these currents are in action at the zenith during only a little more than two-thirds of the semidiurnal interval at the winter solstice, I conclude, (taking all the circumstances into account,) that the value of e will be reduced about one-half at the winter solstice. To find r at the same epoch, we have (p. 27) $6'{\cdot}24 : 1'{\cdot}4 :: r$, at equinox, $: r$, at winter solstice. The approximate values of r and e being thus found, eqs. (1), (2), (6), (7), (13), (14), (17), (19), give the values of R and E at the equinoxes and solstices, as well as of S and D. It will be seen on examining the Table that there is a pretty close correspondence between the determinations of S and D, except in the case in which $m=o$, that is in which the radial currents in existence at noon are supposed to continue in undiminished action during the afternoon. Owing however, to the small quantities neglected in obtaining the formulæ, and to the probability that the assumptions made are only approximately true, we cannot undertake to decide which, among the different suppositions made with regard to the values of m and n, corresponds most nearly to fact. We can only infer it to be highly probable that the radial currents in action at noon pass off almost entirely during the afternoon. Since the value of e is taken the same at each solstice, in eq. (17) $n(e'-e')=0$; the values of S and D given by eqs. (17) and (19) are therefore independent of n, and we can draw no inference with regard to the passing off of the ecliptic currents.

What is particularly important here to remark is that the close correspondence between the determinations of S and D serves as an additional verification of the explanation that has been given of the diurnal and annual variations of declination. We have already obtained a verification of our theory in the fact that the calculated semidiurnal effect of the ecliptic currents at the equinoxes is very nearly equal to half the excess of the midday declination at the autumnal equinox over that at the vernal equinox (p. 207). No account was taken of the residual currents, and we now see (p. 27) that the value of E comes out the same, at the equinoxes, whether these currents be taken into account or disregarded. The value of E for the forenoon is $1'{\cdot}49$; $2\times1'{\cdot}49 = 2'{\cdot}98$. The average values, for forenoon and afternoon, (whether we use Table I or II,) is $2'{\cdot}02$, $2\times2'{\cdot}02=4'{\cdot}04$. The actual deflection is $3'{\cdot}23$.

In the explanation of the deflection of the needle (at 6 A. M.) in the interval between the solstices it was implied that each of the two systems of currents had the same tendency to deflect the needle toward the west at the winter solstice, that it had to deflect it toward the east at the summer solstice ;—that is that the morning currents had the same intensity at these two periods. This idea is however by no means essential to the explanation. It does not accord with fact, for the deflection in the interval between the equinoxes and either solstice would then be half of the whole deflection ; whereas the deflection from the equinoxes to the summer solstice is 3′·47, and from the equinoxes to the winter solstice 1′·43. Both sets of currents are concerned in producing this inequality. As for the morning radial currents they proceed from all the points of the photosphere which are exposed to the sun, but the character of the result may be ascertained by considering the more effective currents. These proceed, at the summer solstice, from the points which lie to the north of the prime vertical ; and at the winter solstice from the points that lie to the south of the prime vertical. The former system of points are nearer to the zenith than the latter, and hence the deflection, so far as it depends on the radial currents, should be greater at the summer than at the winter solstice. It will be seen on referring to the Table on p. 29 that the effect of the ecliptic currents is also greater at the summer than at the winter solstice. The values of E at these two periods are about in the ratio of 4 to 1. The inequality in these two values seems to imply that the excess of the morning currents of the northern hemisphere over those of the southern hemisphere, at the summer solstice, is greater than the excess of the morning currents of the southern hemisphere over those of the northern at the winter solstice. In accordance with this conclusion we have the fact already alluded to that the individual currents of the northern hemisphere (or at least a portion of them) have a much higher intensity in the winter than in the summer. But it remains to be ascertained, by a discussion of the variations of declination observed at some station in the southern hemisphere, whether the currents of the southern hemisphere do not experience a corresponding diminution at the southern solstice.

HORIZONTAL FORCE.

Diurnal Variations.—The curve of the diurnal variations of the horizontal force has two maxima and two minima : the maxima occur about 4 A. M. and 4 P. M., and the minima about 10 A. M and 10 P. M. It may be conceived, for the sake of more ready comprehension, as representing a flood and ebb tide in the ethereal sea of magnetic force ;—the full flood tide lagging behind the sun about 4 hours, at noon and midnight, and the ebb

tide following the flood at an interval of about 6 hours. But it is to be observed that the fall and rise during the night are comparatively slight, and that in the summer months the nocturnal rise sometimes is altogether wanting;—the decrease continuing from 4 P. M. to 10 A. M. The principal minimum is at 10 A. M. to 12 M.; the principal maximum is at 4 P. M. in the summer, and generally at 4 to 6 A. M. in the winter. These phenomena may be referred to the combined action of the two systems of electric currents, radial and ecliptic, by which the corresponding variations of the declination have been explained. In the present case we have the effects of the components of these currents, which cross the meridian at right angles, but in the case of the variations of declination, the effects of the components running in the direction of the meridian. To consider first the effects of the perpendicular components of the radial currents; let us confine our attention for the moment, to the single current proceeding from the point immediately underneath the sun. This is represented by P*h* in Fig. 2 (p. 189); S being supposed to be the point of the photosphere which has the sun in its zenith. During the whole of the forenoon it will be directed toward the west and the horizontal force will be diminished, and during the whole of the afternoon it will be directed toward the east and the horizontal force will be augmented. If we suppose the intensity of the current arriving at P from the point S to be the same for all positions of S on the parallel of latitude AB, P*h* will have a maximum value when the sun is on the prime vertical at A, and again when it is on the same circle, at B, in the afternoon. During the forenoon it will decrease, and vanish altogether at noon. In the afternoon it will go through the same changes in the inverse order. The minimum of horizontal force should therefore occur early in the forenoon and the maximum late in the afternoon.

But in point of fact while the sun is moving from A to M the distance SP is diminishing, and therefore the current propagated from S increasing. The component P*h* is also proportional to cos APS, and therefore at first varies very slowly. It may very well happen therefore that the minimum will occur sometime after the sun is at A, and the maximum sometime before he is at B. Again, instead of a single current proceeding from S, there is actually, according to our theory, a similar current emanating from all the points of the photosphere which are exposed to the suns's action, and the minimum of horizontal force obtains when the resultant of all the actions of these innumerable currents, directed toward the west, begins to decrease. At what hour this will occur cannot be determined, until we have ascertained the law of variation of the currents traversing arcs of different lengths. We can only say that it doubtless will occur before the middle of the forenoon; and the maximum of horizontal force, about the

same hour in the afternoon. The tendency of residual currents, so far as they may act, will be to make the minimum happen later in the forenoon, and the maximum later in the afternoon.

But the ecliptic currents play a no less important part than the radial, which we must proceed to consider. At noon, or near that hour, these currents will be perpendicular to the meridian, but about 6 A. M. and 6 P. M. their obliquity to the meridian will be at its maximum. These currents will always tend to diminish the horizontal force, and will act with the greatest effect when their obliquity is the least; since their components in the direction perpendicular to the meridian will then be the greatest, and for the additional reason that the currents themselves are then the most intense. Their tendency therefore is to make the minimum of horizontal force fall at or near the hour of noon, and the maximum at about 6 A. M. and 6 P. M. Acting as they do, in conjunction with the radial currents, they will have the effect to make the minimum fall later in the forenoon, and the subsequent maximum later in the afternoon. The relative effect of the two sets of currents, it is reasonable to suppose, should however be different in different seasons, and hence the hours of maxima and minima vary somewhat, as they are known to do, from one season to another.

To ascertain the comparative semidiurnal effects of the ecliptic and radial currents, denote them respectively, by R and E, and let m= variation of horizontal force from 6 A. M. to noon, and m'= variation from noon to 6 P. M; then $m=R-E$, $m'=R+E$; whence $E=\frac{m'-m}{2}$, and $R=\frac{m'+m}{2}$ ...(20). It is to be observed that R is equal to the effect of the component, perpendicular to the meridian, of the resultant of the radial currents at 6 A. M., and nearly equal to that of this resultant itself; and that E is nearly equal to the effect of the ecliptic currents at noon. We here leave out of view the residual currents. To take them into account let r= effect on horizontal force of residual radial currents at noon; r'= same at 6 P. M.; e= effect of residual ecliptic currents at noon; e'= same at 6 P. M.; k and l= effects of nocturnal radial and ecliptic currents respectively, in action at 6 A. M. (or rather of that portion of them which passes off during the forenoon); pr= the proportion of residual and qe= the proportion of ecliptic currents that pass off during the afternoon, then

$$m=R-E-r-e+k+l,\ m'=R+E+r'+pr+qe-e' \quad \ldots \quad (21),$$

$r=r'$ (nearly) and $e=e'$ (nearly); k and l are comparatively small; the values of p and q lie between o and 1, but are probably greater than $\frac{1}{2}$. We have then,

$$E=\frac{m'-m}{2}-\frac{r(2+p)+qe}{2}\ \text{(nearly)},\ R=\frac{m'+m}{2}-\frac{pr+e'q-2)}{2}$$

(nearly) . . . (22). From which it appears that both E and R are less than as determined by eqs. (20), and that the diminution of E, by reason of the residual currents, exceeds that of R by the quantity $r+e$.

The nocturnal variations of the horizontal force, like the diurnal, are the joint effect of the two systems of currents; and the tendencies of each system are the same as during the day. The intensity of the currents is only diminished; that of the radial currents because these currents now proceed from the portion of the photosphere which is diametrically opposite to the sun (p. 191) and are weakened by the currents proceeding from the points underneath the sun; and that of the ecliptic currents because they come from the southern hemisphere, and at midnight have traversed an arc of 180°. In the winter of northern latitudes the radial currents which proceed from the part of the photosphere diametrically opposite to the sun, come from points in the northern hemisphere, and hence the nocturnal rise of horizontal force should be greatest in the winter. In the summer the corresponding currents, proceeding from points in the southern hemisphere, in conjunction with the nocturnal ecliptic currents, generally occasion a slight augmentation of horizontal force after midnight; but in some instances they only suffice to check the tendency to a diminution, from the action of the radial currents propagated from the points underneath the sun, and from the gradual subsidence of the residual currents consequent upon the sun's action during the day. (See curves of variations of horizontal force during the summer months, at Toronto and Hobarton, in the Hobarton Observations, Vol. I, p. 54.)

Annual Variations.

These as observed at Toronto, may be studied in Table III, on the opposite page.

The principal annual variations are: 1, An inequality in the diurnal range, the amount of the range being considerably greater from April to September than from October to March: 2, A change in the hours of the maxima and minima; especially in the hour of the principal minimum, which is at 9 or 10 A. M. from April to October inclusive, and at 11 A. M. or 12 M. from November to March inclusive: 3, A variation in the mean monthly value of the horizontal force; the maximum being in June and the minimum in December. Preparatory to the explanation of these variations let us compare the mean semidiurnal effects of the radial and ecliptic currents in the different months of the year. For this we have (disregarding the residual currents) the equs. (20) (p. 33), which give the following results:

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
R	10·5	18	29·5	53·5	56	58·5	50	48	48	23	3	29
E	108·5	82	97·5	78·5	39	48·5	36	43	99	72	104	96

These values of E show approximately, the comparative mean intensities of the ecliptic currents at noon, in the several months, and the values of R the intensities of the radial currents at 6 A. M. or 6 P. M. These numbers also represent the relative semidiurnal effects of the two systems of currents. It appears that the intensity of the radial currents is twice as great in June as in December, while the intensity of the ecliptic currents is twice as great in December as in June. It would naturally be supposed that the radial currents would be more intense in summer than in winter; and it is to this fact that we are to attribute the increase in the diurnal range of the horizontal force from winter to summer. If we take the range from 6 A. M. to 6 P. M. it will be due entirely to the action of the radial currents, and will be proportional to R. The range from the morning minimum to the afternoon maximum should be greater than 2R, at the summer solstice, because the greatest action of the radial currents to diminish the horizontal force obtains a certain length of time after the passage across the prime vertical (p. 32), and the greatest action to augment the horizontal force before the passage across the prime vertical in the afternoon.

The annual variation of the hour of the principal minimum is another consequence of the change that occurs during the year

TABLE III.

Mean Diurnal Variation of the Horizontal Force in each Month of the Year, at Toronto; derived from Hourly Observations in the Years 1843 *to* 1848.

Astron. time at Toronto.	Jan.	Feb.	Mar.	Apr	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Apr. to Sept. inc.	Oct. to Mar. inc.	Ann. Means
	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00
0h	003	008	008	026	057	054	055	053	068	039	014	000	052	010	026
1	036	039	043	060	100	091	074	098	149	068	040	022	095	039	062
2	069	069	084	099	144	138	126	141	205	110	084	067	142	079	105
3	108	107	129	143	169	168	148	171	245	134	112	096	174	113	138
4	141	126	147	164	181	178	161	175	253	155	128	127	185	135	155
5	136	130	149	173	174	180	158	173	238	151	120	126	183	133	153
6	122	108	135	158	152	161	141	144	215	134	121	125	162	122	137
7	117	107	124	129	144	139	130	122	190	122	120	124	142	117	125
8	112	102	118	108	120	112	106	115	178	111	113	108	123	109	111
9	101	087	103	096	116	092	095	103	171	102	104	097	112	097	100
10	099	085	088	090	089	082	078	091	164	097	086	086	099	088	089
11	090	080	074	053	081	068	066	069	151	085	085	084	081	081	076
12	069	064	071	060	064	062	051	068	139	083	075	060	074	068	066
13	066	046	060	047	052	052	048	051	123	061	072	057	062	058	055
14	057	042	064	005	041	051	041	052	115	047	079	069	051	058	049
15	071	049	063	020	029	041	030	039	111	073	084	072	045	067	051
16	085	052	070	051	045	041	027	045	134	061	091	068	057	069	058
17	091	065	069	059	031	044	030	045	131	091	102	054	057	077	062
18	101	072	076	051	040	044	041	048	119	088	115	067	057	085	066
19	103	036	060	038	042	039	044	036	091	058	103	078	048	071	055
20	088	026	038	029	028	030	038	023	064	024	076	067	035	050	037
21	060	023	015	015	002	014	016	001	020	000	030	045	011	027	014
22	024	008	005	000	000	000	000	000	000	003	011	022	000	010	000
23	000	000	000	001	020	017	024	016	017	004	000	006	016	000	003

in the relative intensities of the two systems of currents. In the summer the radial currents are more efficient than the ecliptic, but in the winter the ecliptic currents are much more efficient than the radial; and the tendency of the radial currents is to produce a minimum early in the forenoon, and of the ecliptic currents to produce a minimum at noon. It follows therefore that the actual minimum should occur later in the forenoon in the winter than in the summer.

The increase in the mean monthly value of the horizontal force from the winter to the summer solstice, finds its explanation in the varying action of the ecliptic currents. In the course of a single day the diminution and augmentation of the horizontal force that result from the action of the radial currents balance each other, and hence the variations in the mean daily values of this force from one month to another, must be ascribed to the annual variation of the ecliptic currents. Now these currents have the highest intensity in the winter (p. 35), and always tend to diminish the horizontal force; therefore the mean daily value of this force should be less in winter than in summer. The following numerical results furnish a strong confirmation of the truth of this explanation. Mean annual intensity of the ecliptic currents, at Toronto, for the years 1843–8, (or mean of the values of E in Table on p. 34)=75·3; mean intensity of same for the months of Jan., Feb., March, Sept., Oct., Nov., and Dec.= 94·1; mean intensity for April, May, June, July, and Aug.= 49·0. From which 94·1−75·3=18·8; 75·3−49·0=26·3. Again mean value of horizontal force, at Toronto, from 1845 to 1851, (allowing for secular change, Toronto Observations, vol. ii, p. 91)=3·53088; mean horizontal force for the months of Jan., Feb., March, Sept., Oct., Nov., and Dec. =3·52973; mean horizontal force for April, May, June, July, and Aug. =3·53250. Whence 3.53088−3·52973= ·00115; 3·53250−3·53088=·00162. Now ·00162÷·00115= 1·409; and 26·3÷18·8=1·400. From Sept. to March, inclusive, the ecliptic currents are the most energetic, and have pretty nearly the same intensity. The intensity in April is as high as the lowest intensity during this interval. If we include April in the months of highest intensity, that is compare January, February, March, April, September, October, November, December, with May, June, July, August, we have a still closer agreement of results, for the quotients come out respectively 2·00, and 1·99.

VERTICAL FORCE.

Diurnal and Annual Variations.—It is first to be observed that, agreeably to the general theory which is under consideration, the diurnal and annual variations of the vertical force are to be referred entirely to variations in the intensity of the ecliptic currents. The radial currents which diverge from any point of the

photosphere, and pass near the zenith on opposite sides, will neutralize each other in their action on the vertical force, since they will be of equal intensity. But the ecliptic currents that lie on opposite sides of the zenith will in general be of unequal intensity, and will affect the vertical force in proportion to the amount of their difference of intensity. Any variations that may take place in this difference will accordingly be attended with variations in the intensity of the vertical force. If we consider the entire set of ecliptic currents above the horizon it will be seen that the perturbations of the vertical force will be chiefly produced by the currents which are remote from the zenith; for the proper tangential force of any single current will lie in the plane of the horizon if the current passes through the zenith, but will lie in the vertical line if the current is in the horizon, and will then be wholly effective in disturbing the vertical force. The currents which are below the horizon have the same tendency as those which are above it, but they have less effect. In the case of the horizontal force the currents above and below the horizon have opposite tendencies. If it should seem improbable that the differences of intensity of the ecliptic currents should be the occasion of sensible disturbances of the vertical force, when the direct effects of these currents on the horizontal force are slight, it is to be observed, in addition to the distinction just noticed, that the effect on the vertical force of two currents on opposite sides of the zenith, at the distance of 30°, is one half of the entire force due to the difference of the currents; also that the surface of the atmosphere crosses the plane of the horizon under a small angle, and hence that the portion of the photosphere which is in most effective action is very large in comparison with that in the vicinity of the zenith which is principally concerned in modifying the horizontal force. If we suppose the height of the atmosphere to be about 60 miles 61 (miles) its surface will cross the plane of the horizon under an angle of 10°, and the length of an arc extending 45° from the zenith will be about 60 miles while the length of its continuation down to the horizon will be about 640 miles.

In studying the effects of the ecliptic currents on the vertical force we must observe that any current which passes to the south of the zenith tends to augment the vertical force, and any current which passes to the north to diminish it; again that any current which, coming from the north, passes to the east of the zenith tends to augment the vertical force, and any current which, coming from the same quarter, passes to the west of the zenith tends to diminish it; also that in the case of currents coming from the south the effects are just reversed. The variations experienced by the vertical force on any particular day must depend on the law of variation of the ecliptic currents along the circle of excitement (p. 200), on the

law of variation of the individual currents with the distance from the points at which they originate, and on the position of the diurnal circle traced by the zenith among the ecliptic currents, or on the sun's distance from the zenith at noon. The previous discussion has not revealed these laws, it has only served to confirm the natural supposition that the intensity of each individual current decreases in both directions from the point of original excitement, and made known the comparative intensities of the currents which pass through the zenith at noon in the different months of the year. Avoiding, for the present, all speculation as to the mode of excitation of the ecliptic currents, the most probable supposition that can be made with regard to the relative intensities of the different currents developed at any instant, is that at any particular instant the current which is at any given distance from the point underneath the sun has the same intensity as the current which passes through the zenith on that day on which the zenith is at the same given distance from the point under the sun. This is equivalent to supposing that the intensity of a current depends in every instance, entirely on the distance of the point at which it originates from the point of the photosphere which has the sun in its zenith. It would seem that this should be the case unless there are inequalities in the photosphere. Adopting this supposition, then, we may infer from the results given in the Table on p. 34 that, beginning at a point about 20° from the sun, the ecliptic currents gradually increase in intensity until we reach the distance of about 45°, but that from this point to 67° they retain pretty nearly the same intensity. Beyond 67°, toward the horizon, we must conclude that the currents in question, if they have their origin in any direct solar action, in all probability diminish in intensity. Whatever may be the law of distribution of the ecliptic currents over the photosphere, at any moment, it is important to remark that, except as there may be inequalities in the photosphere, it should be the same at all seasons; and therefore that the laws of the diurnal variations of the vertical force should be the same at corresponding seasons in both hemispheres (*e. g.* at Hobarton as at Toronto).

If the above conclusions are correct then the difference of intensity of currents on opposite sides of the zenith must be greater at 6 A. M. or 6 P. M. than at noon, and hence there should be a tendency to a diminution of the vertical force from 6 A. M. to noon, and to an augmentation from noon to 6 P. M. But if the ecliptic currents after their excitation subside gradually, from hour to hour, we shall have in action at noon the differences of all the residual currents consequent upon the forenoon excitation, and at 6 P. M. the differences of all the residual currents resulting from the sun's action during the whole day. The tendency of these residual currents will be to make the vertical force greater

at noon than at 6 A. M., and greater at 6 P. M. than at noon, unless the currents in action at noon should pass off entirely during the afternoon. The effect of the residual currents at noon being approximately due to the sum of a series of successive differences, were it not that these differences have decreased according to some unknown law, would be approximately proportional to the difference between the ecliptic currents at 6 A. M. and at noon. In the play of the two tendencies just mentioned I conceive that we have the general explanation of the diurnal variations of the vertical force.

TABLE IV.

Mean Diurnal Variation of the Vertical Force in each Month of the Year, at Toronto; derived from Hourly Observations in the Years 1843 *to* 1848.

Astron. time at Toronto.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Apr. to Sept. inc.	Oct. to Mar. inc.	Ann. Means.
	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00	·00
0ʰ	007	008	006	015	011	002	015	028	036	025	013	016	017	013	015
1	011	011	011	024	017	004	018	033	046	031	018	022	023	017	019
2	515	017	017	028	026	011	027	040	052	035	026	030	030	023	025
3	019	019	022	033	032	017	034	046	056	037	027	030	035	026	029
4	016	022	023	037	037	021	040	052	056	036	031	030	040	026	031
5	019	023	025	038	042	023	044	052	051	039	029	028	041	027	032
6	018	022	026	037	039	023	041	048	050	038	027	029	039	027	031
7	019	023	028	034	038	022	038	044	048	040	031	030	036	028	031
8	019	023	025	030	033	018	032	039	044	038	028	028	032	027	028
9	017	018	023	028	029	015	023	030	038	032	021	028	031	023	023
10	016	017	015	021	021	011	021	022	031	029	016	025	020	021	018
11	012	014	007	008	016	008	014	009	029	024	009	020	013	014	012
12	008	007	002	004	007	004	007	005	019	014	008	010	007	008	006
13	007	001	000	003	008	000	001	001	007	009	006	010	002	006	002
14	002	000	000	000	000	001	000	003	000	009	002	011	000	004	000
15	001	001	000	001	003	000	003	000	001	005	003	013	001	004	001
16	001	000	000	003	011	006	007	007	004	000	000	001	005	000	001
17	001	002	001	006	014	011	014	019	009	005	002	000	011	002	005
18	004	004	007	014	020	014	017	028	017	008	005	005	017	005	010
19	005	004	010	011	017	013	018	029	022	020	007	003	017	008	011
20	007	015	011	012	017	011	018	027	020	026	009	009	016	013	013
21	001	005	006	012	013	007	017	024	021	019	006	009	015	008	010
22	000	002	003	011	009	004	014	024	024	020	007	012	013	007	009
23	003	002	002	011	009	001	014	024	028	019	009	013	014	008	009

"The diurnal variation of the vertical force at Toronto in both seasons, *i. e.*, from April to September inclusive, and from October to March inclusive, is a double progression, having two maxima and two minima. The principal maximum takes place two hours earlier from April to September than from October to March, viz, at 5^h from April to September, and at 7^h from October to March. From this maximum the diminution is progressive to the principal minimum, which also occurs earlier from April to September than from October to March; *i e.*, between 14^h and 15^h (2 A. M. and 3 A. M.) from April to September; and at 16^h (4 A. M.) from October to March. The secondary minimum is at 22^h (10 A. M.) in both seasons. The range of the diurnal variation is greater

during the six months when the sun is north of the equator, or from April to September, than in the opposite season."

We have seen (p. 38) that there are two causes in operation tending to produce a diurnal variation of the vertical force; in the forenoon they are opposed to each other but in the afternoon conspire to augment the vertical force. It would seem that at Toronto the tendency to an increase, by reason of the residual currents, prevails early in the forenoon over the tendency to a diminution, resulting from the decrease in the difference of intensity of the currents on opposite sides of the zenith. Later in the forenoon the other tendency prevails and a secondary minimum results. The range from 6 A. M. to 6 P. M. is entirely due to the residual currents.

Let c= effect on the vertical force of the difference of intensity of the ecliptic currents on opposite sides of the zenith, at noon; c'= same at 6 A. M. or 6 P. M.; s= effect of sum of residual currents at noon, resulting from forenoon excitation, or of residual currents at 6 P. M. resulting from afternoon excitation, m= rise of vertical force in the interval from 6 A. M. to noon; and m'= same from noon to 6 P. M.; then, supposing the residual currents in action at noon to continue without diminution until 6 P. M.,

$$s-(c'-c)=m,\ s+(c'-c)=m';\ s=\frac{m+m'}{2},\ c'-c=\frac{m'-m}{2}\ldots(23).$$

Or supposing the residual currents at noon to pass off entirely during the afternoon,

$$s-(c'-c)=m,\ c'-c=m';\ s=m+(c'-c)\ \ldots\ (24).$$

The following Table contains the values of s and $(c'-c)$ for each month in the year, computed from equations (23) and (24), and the means of these values.

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug	Sept.	Oct.	Nov.	Dec.
s	7	9	9·5	11·5	9·5	4·5	12	10	16·5	15	11	12
s	14	18	19	23	19	9	24	20	33	30	22	24
Means,	10·5	13·5	14·25	17·25	14·25	6·75	18	15	24·75	22·5	16·5	18
$c'-c$	4	5	10·5	10·5	18·5	16·5	14	10	−2·5	−2	3	1
$c'-c$	11	14	20	22	28	21	26	20	14	13	14	13
Means,	7·5	9·5	15·25	16·25	23·25	18·75	20	15	5·75	5·5	8·5	7

The means, which answer approximately to the supposition that the residual currents in action at noon are reduced in intensity one half at 6 P. M., probably correspond most nearly to fact.

If we inquire into the phenomena of the variations of the vertical force at other stations, we find that there is a considerable diversity. For example at Philadelphia, the maximum is generally at 2 P. M., and the force decreases from this hour to about midnight, then increases until 2 A. M., and decreases again until 4 to 7 A. M.; from the hour of the morning minimum to that of the afternoon maximum it steadily increases, but the increase is much less rapid toward 10 A. M., especially in the winter, than before or after that hour. At Toronto this falling off in the augmentation amounts to the production of a secondary minimum.

"At Hobarton there is one decided minimum throughout the year, and in the winter of the southern hemisphere but one maximum. The minimum is at 7 A. M. in the summer, and at 9 A. M. in the winter. The winter maximum falls at 3 P. M. In the summer there are two maxima, at noon and at 6 P. M., the latter maximum being rather superior. Intermediate between these two afternoon maxima, is a small secondary minimum. The range of the diurnal variation is nearly the same at both periods of the year, and amounts to about four parts in a thousand of the whole vertical force." Both at Toronto and Philadelphia the amount of the diurnal range is greater in the summer than in the winter. This diversity in the phenomena at different stations indicates the existence of inequalities in different parts of the earth's photosphere, by reason of which the law of variation of the intensity of the system of primary ecliptic currents excited at a given hour by the sun is modified at particular localities, or what is more probable, the currents which have been developed subside more rapidly at some stations than at others. Thus we may explain the phenomena at Philadelphia by supposing that the residual currents pass off very rapidly in the afternoon but are particularly effective in the forenoon. The residual currents having passed off, the nocturnal currents come into sensible action, producing a secondary maximum at 2 A. M.; but at Toronto and Hobarton these currents serve only to modify the diminution of the vertical force, resulting from the slow subsidence of all the currents which are in operation at sunset.

IRREGULAR VARIATIONS.

While the periodical variations of the magnetic elements may be traced to periodical changes in the intensity and direction of propagation, of two systems of electric currents, radial and ecliptic, developed by some action of the sun in the upper atmosphere, the irregular variations so-called may be referred with a high degree of probability, to the occasional excitation of the same two systems of currents. The following are among the more prominent indications of the irregular action of the radial and ecliptic currents.

1. It appears from Colonel Sabine's discussion of the irregular variations as observed at Toronto, that the general effect on the horizontal force is a diminution, and the ecliptic currents must always have this effect; 2. The westerly variations of declination have a maximum both in number and value, at Toronto, about 7 or 8 A. M., and a minimum about 9 P. M., while the easterly variations have a maximum about 9 P. M., and a minimum about 3 P. M. These facts may be explained by the occasional action of radial currents, if we assume that the greatest liability to their excitation is always on the meridian which is from 4^h to 5^h to the west of the sun, and to the south of the latitude of

Toronto, for on this assumption the radial currents will, at 7 or 8 A. M., lie in the meridian, and proceeding from the south, will deflect the needle farther to the west than at any other hour, when the supposed meridian of maximum excitation will deviate more or less from the meridian of the station. Again on the same supposition, the meridian in question, preceding the sun some four hours, the currents originating on it and propagated to the zenith of the station will again lie in the meridian of the station at about 8 P. M., but they will now come from the north and deflect the needle toward the east. At the hour of the minimum of easterly disturbance, 3 P. M., when the westerly disturbance is also reduced to nearly its minimum of frequency and value, the points of especial excitation are in the vicinity of the prime vertical, and it is only the feebler currents proceeding from points remote from the meridian of maximum disturbance that will be inclined under a small angle to the meridian of the station; 3. In striking confirmation of the explanation just given, the hours of maximum disturbance of the horizontal force, in excess and defect, are removed about 6 hours from the hours of maximum disturbance in declination. Thus the disturbances augmenting the force are most numerous from noon to 4 P. M., when the points of most frequent excitation are near the prime vertical toward the west, and their radial currents are in the position to have the greatest effect in augmenting the horizontal force. Again the disturbances in defect predominate especially at from 10 P. M., to 4 A. M., when the points of most frequent disturbance are near the prime vertical toward the east, and when the current propagated from them to the zenith should have the greatest effect in weakening the force. Also the minimum of disturbance in excess is at the hour of the maximum of disturbance in defect, and the minimum of disturbance in defect is at the hour of the maximum disturbance in excess. The disturbances in defect preponderate over those in excess at all hours; in this fact, as already intimated, we have an evidence of the operation of occasional ecliptic currents. These should have a high intensity in the afternoon, when the disturbances in excess are the most frequent, and may accordingly cause the disturbances in defect to preponderate then. The preponderance is but slight (1·3 to 1 for the years 1841–2). 4. The westerly disturbances of declination decline in numbers and value for about 12 hours before and after the maximum, with but one or two slight irregularities; and the same is true of the easterly disturbances (except that there is slight increase from 6 hours to 12 hours before the hour of maximum). This fact indicates that the points of the photosphere at which the irregular excitation occurs extend some 6 hours from the meridian of maximum excitement in both directions. Other indications might be pointed out but these will suffice for our present purpose.

As to the exciting cause of these irregular currents, the occasional ecliptic currents can only be referred, it would seem, to paroxysmal disturbances of the sun's magnetic force; which disturbances, from what we have already seen (p. 185), must be in some way connected with those mighty processes going on at the sun's surface that reveal their action by giving rise to the appearance of dark spots on the disk. The irregular radial currents cannot be ascribed to the sudden augmentation of some impulsive force emanating from the sun, for such a force would have its greatest effect on the meridian over which the sun was, and thus the maximum of westerly disturbance of declination would be at noon, instead of 7 or 8 A. M. They may be supposed to have their origin in occasional electric discharges occurring between the upper and lower atmosphere, attended with the flow of horizontal currents from or toward the points of discharge, but the difficulty still remains, of assigning any plausible reason why the meridian of greatest excitement should continually precede the sun 4 or 5 hours in its diurnal course, and the disturbance of the electric equilibrium decrease in both directions from this meridian. We can only conjecture that this fact might possibly be attributed to variations of atmospheric humidity. But there is another and more plausible supposition that may be made; it is that the irregular radial currents have their origin in the sudden arrival at certain points of the upper atmosphere, from time to time, of some material emanation radiating from the sun's surface; and either by its force of impact or by an electric action, developing electric currents diverging in every direction over the photosphere. If we conceive the supposed material emanation from the sun to have, when it reaches the earth, the same velocity as the earth in its orbit, then, confining our attention for the moment to the circle of intersection of the plane of the ecliptic with the surface of the photosphere, the radiant matter would continually arrive in the direction normal to the surface at the point of this circle which is 45° to the west of the sun. If now we consider portions of the photosphere to the north or south of the ecliptic, it will be seen on a little reflection that the direction of approach will deviate the least from the normal at the points which are situated on the great circle traced through the point of the ecliptic 45° to the west of the sun and perpendicular to the ecliptic. From this circle, in both directions parallel to the ecliptic, the obliquity of the solar emanations to the surface of the photosphere will gradually increase. If now we assume the velocity of these material radiations to be less than that of the earth in its orbit, the circle just mentioned will be farther to the west; the limit of distance from the sun being 90°, answering to the supposition that the radiant velocity is zero. To satisfy the results of observation this circle must be conceived to be from 60° to 75° to the west of the sun. We shall have a simple general

conception of the entire action of the solar emanations if we observe that the effect is the same that it would be if the sun were constantly from 60° to 75° to the west of its actual position, and the earth had no orbitual motion.

This view of the origin of the irregular perturbations of the directive force of the magnetic needle connects theoretically these phenomena with the appearance and disappearance of the solar spots; since it is natural to suppose that the material radiations from the sun are connected with those disturbances at the surface of the sun which are attended with visible effects. It may be conjectured that these perturbations may originate in electric disturbances in the upper atmosphere attendant upon the earth's encounter with the meteors that lie in its path; but this view appears to be irreconcilable with the fact that the circle of greatest excitement is only 60° to 75° to the west of the sun, instead of 90°. Besides it furnishes no link of connection between the perturbations and the sun's spots.

I have shown, in previous publications, that in the idea of material emanations from the sun's surface, according to a certain law, we have an adequate explanation of the Zodiacal Light; also that the matter thus radiated from the sun is probably the substance of terrestrial Auroras; and that it plays an important part in Cometary phenomena.

Note.—The same effects which in the present discussion have been referred to electric currents developed in the upper atmosphere, would be produced by currents circulating at the earth's surface in the opposite direction; that is, the one set converging toward the points of the surface which are exposed to the sun, and the other running from west to east, and at the outset parallel to the ecliptic. But there are several objections to the adoption of this view as a physical fact: *e. g.*, now that it has been established that evaporation does not disturb the electric equilibrium at the surface of the earth, there is no known action which can give rise to currents of positive electricity converging to the points on which the sun acts, and the tendency of any supposed electric force propagated from the sun would be to create diverging instead of converging currents. Again if the sun has the tendency to develop in the crust of the earth currents running from west to east, then the sun cannot have been the exciting cause of the normal magnetism of the earth, since the permanent currents run from east to west. Other objections might be stated.

The fundamental conception of two systems of electric currents which has served for the explanation of the disturbances of the magnetic needle, when transferred to the contiguous molecules of bodies conceived to be surrounded with electric atmospheres, appears to afford a sufficient basis for a satisfactory *dynamical* theory of frictional and galvanic electricity, and of the principle of magnetism in its various aspects.

Art. IV.—*On the Periodical Rise and Fall of the Lakes;* by Major Lachlan.*

(Concluded from vol. xix, p. 175.)

Leaving any further remarks on the foregoing for a future probable opportunity, I may here briefly observe, that I have long been persuaded that the severity of our winters is mitigated by the proximity of the Lakes, and is not so much owing to the prevalence of winds from the northwest, as a mere northerly point of the compass, or to the remarkable curve of the great isothermal line in this part of the globe, as to the winds alluded to sweeping down from a *more elevated region*, many parts of the extensive mountainous tract of country stretching in that direction being perhaps thousands of feet above the level of Lake Superior, and even the latter not being less than 600 feet above that of the ocean.

Nearly the whole of the conflicting evidence bearing on the various points at issue having been adduced, I proceed to state freely, yet as briefly as possible, the mode of proceeding adopted by me, in my endeavor to arrive at the convictions to which I have been thereby led with respect to each of the three questions to be determined.

To commence with the first of these, namely, the traditional report of there being a septennial rise and fall in the waters of the great Lakes, &c., I have to remark, that being unwilling to admit any assertions on so interesting and mysterious a phenomenon without thorough examination and comparison with facts, I, after much reflection, determined to attempt to form from the materials in my possession a general comparative tabular view of the positively known, and, failing that, generally acknowledged, periods of elevation and depression throughout the whole of the Lakes, during the longest ascertainable series of years; in the hope of thereby arriving at something like an approximation to the real state of the matter: but after laboring long and patiently at the unsatisfactory task, I was at last obliged to abandon it and confine my synopsis to Lake Erie alone, and even then to add various "Miscellaneous Remarks" for the mention of any apparent coincidence, or otherwise, in the state of the other Lakes; and in this I continued to persevere till, after much labor, I so far succeeded, as is shown in the following copious yet imperfect Table, exhibiting not only the various progressive and retrogressive annual fluctuations in the level of that particular Lake during a course of sixty-three years, as vouched by the different highly respectable authorities named, but also proving, incidentally, how far that long received traditional phenomenon, the rise and fall of the Lakes generally every seven years, is in accordance with the evidence furnished by recorded facts:—

* From the Canadian Journal, July, 1854.

Comparative view of the Rise and Fall of the Waters of Lake Erie, for sixty-three years in succession, as far as ascertained from the best sources of information within reach.

Date.	Comparative Level.	Authorities.
1790	1*st maximum*; being 5 ft. 6 in. above lowest level.	Prof. Hall, Higgins, Whittlesey, Mather, &c.
1791		
1792–94	No information regarding these years.	
1795, '96	1*st minimum*. Level described as low, but not exactly stated.	Weld, Whittlesey, &c.
1797–99	Rising, but amount not stated.	Do. do.
1800–2	High. 2*nd maximum*.	Higgins, Houghton, Whiting, &c.
1803–5	No information whatever.	
1806	Level low.	Whittlesey, &c.
1807–9	Rising. No information.	
1810, '11	2*nd minimum*. Reported as 6 feet below 1838.	Do.
1812–14	Waters rising.	Houghton, Higgins, Dearborn, Whiting, &c.
1815	3*d maximum*, but 2 ft. less than 1838.	Do. do. do.
1816	Same as last year.	
1817, 18	Falling.	Houghton, Higgins, &c.
1819, 20	3*d minimum*, or zero.	Houghton, Higgins, Whittlesey, and Whiting.
1821	Rising rapidly.	Do., do., do., Dearborn, M'Taggart.
1822	Do., but still low.	Do.
1823	Up to average or mean.	Do.
1824–26	Gradually rising, to within 2 feet of maximum of 1838.	
1827, '28	4*th maximum*, reckoned as high as in 1815.	Houghton, Higgins, Whiting, Dearborn, M'Taggart, &c.
1829	Still high.	
1830	As high as in 1828.	
1831	Subsiding rapidly.	Whiting, &c.
1832, '33	4*th minimum*, though only down to average.*	Whittlesey, Higgins.
1834	Rising.	
1835	2 feet 10 inches below 1838.	Houghton, Higgins, Mather, Whiting, Whittlesey, &c.
1836	1 foot 8 " " "	
1837	0 " 9 " " "	
1838	5*th maximum*. 5 feet 3 inches above zero.	Amer. Jour. Sci., Prof. Dewey, Buffalo Express.
1839	3 feet 8 inches above zero.	
1840	3 " 5 " " "	
1841	3 " 1 " " "	
1842	3 " 7 " " "	
1843	2 " 8 " " "	
1844	2 " 11 " " "	
1845	3 " 0 " " "	
1846	2 " 0 " 5*th minimum*.	
1847	2 " 6 " " zero.	
1848	2 " 2 " " "	
1849	3 " 1 " " "	
1850	2 " 8 " " "	
1851	2 " 11 " " "	
1852	Rising rapidly.	
1353	6*th maximum*; very high, as in 1832.	
1854		

N. B.—In contrast to Lake Erie, from 1846 to 1852, Lake Ontario was as follows at mouth of Genessee River :—

2 ft. 3 in. from top of dock.
1 " 1 " "
2 " 1 " "
1 " 9 " "
1 " 5 " "
1 " 11 " in July, "
1 " 2 " "

* By *average* is meant the *mean*, or half-way between the two extremes—say 2 feet 9 inches below the maximum of 1838.

Miscellaneous Remarks.—From 1788 to 1790, the Lakes generally, and Lake Erie in particular, stated to have been as high as in 1838, at which time, according to different authorities, compared with the lowest level known, it was estimated at 5 ft. 3 in., 5 ft. 4 in., and even 6 ft.; and Prof. Hall mentions evidence of a higher level than in 1838, in ridges and submerged trees.

During 1795 and 1796, *Lake Ontario* described as so high as to have drowned orchards near Kingston of 30 years growth, while the gravelly beach of Lake Erie near Cleveland was used as a road, and continued so for many years afterwards.

In 1798, Lake Erie reported as higher than in 1796.

Waters of Lake Erie, and of the others generally, high from 1800 to 1802; and the level loosely estimated as similar to that of 1827.

1806, level reported, in general terms, as low.

The level of 1810 is compared with the floods of 1790 and 1838: which would give about 2 ft. 9 in. below the mean level.

Gen. Dearborn states, from personal knowledge, that *Lake Erie* was, in 1814, more than 2 feet higher than in 1813, and that the river Detroit was unusually high during that and the following year, and much land submerged; Dr. Houghton describes the Detroit as high in 1814 and 1815; and Mr. Higgins the upper Lakes as full in 1814, and the central and lower Lakes in 1815.

In 1815, like previous year, Detroit and St. Clair Rivers unusualy full, and the rise of Ontario regarded as generally about 2 feet higher than the other Lakes.

In 1817 and 1819, an ebb and flow of from 14 to 18 inches, noticed at Green Bay by Major Storrow, and in 1820 by Mr. Schoolcraft, and in 1827 by Col. Whiting.

In 1819 and 1820, the central and lower Lakes described by Messrs. Higgins and Whittlesey as unusually low; while Col. Whiting and Dr. Houghton state that the Detroit River had resumed its usual level.

Lakes Huron and Erie described as having resumed their usual level during 1811.

In 1823 a rapid rise of 2 feet, from 5 to 3 feet below 1838.

In 1824, ice for a time blocked up outlet of Lake Huron, and River Detroit in consequence fell rapidly 10 feet; and a great depression took place in Lake Erie and Ontario, while the pent-up waters flowed back on Lake Huron. In other respects, the Lakes appear to have been in their usual state. In 1831, a similar occurrence.

In 1827 and 1828, Lake Ontario (and other Lakes) 2 feet higher than in 1820; and yet, according to Mr. M'Taggart (who estimates extra height between 2 and 3 feet), the rivers flowing out of them did not appear to be affected thereby; while Gen. Dearborn states that, though Lakes Erie and Ontario were so high, Lake Superior was lower than ever known before. In 1830, the level of Lake Erie rated at 2 feet above 1819.

In 1831, Lake Erie fell temporarily between 3 and 4 feet. (See also 1824.)

In 1835, Lake considered 1 ft. 8 in. higher than in 1819; and afterwards in 1842.

In 1836, level the same as in 1830, and subsequently in 1853, and 1 ft. higher than the previous year. *N. B.*—The figures in the "Comparative Level" column to 1838 from Mr. Higgins.

In 1838, Lake stated by Higgins to be 5 ft. 3 in. above 1819, and by Buffalo *Advertiser* 5 ft. 4 in. in June and 5 ft. 9 in. in August: and according to Dr. Houghton, it might be 6 ft.: much land overflowed and trees of 100 years growth killed. Lake Ontario said to be 6 ft. 10 in. above 1825, Lake Huron higher than for two centuries, Michigan 6 ft. higher than in 1820, and Superior said to be 3 ft. higher than usual, and 1 ft. above 1837. By February, 1839, Erie had fallen to 3 ft. 8 in. (see also 1827), and in 1840 it was higher than for 23 years before, with the exception of 1838. *N. B.*—The levels in figures from 1839 to 1851 from Buffalo *Advertiser* of April, 1851.

In 1844, all the Lakes considered low; but during the night of 18th October, Lake Erie suddenly rose temporarily at Buffalo 13 ft. 8 in. above the harbor zero, caused by a great storm. In 1845, a sudden rise and fall of Lake Ontario, caused (according to Prof. Dewey) by a tornado, with water-spout, thunder, and hail. In that year, however, Lakes Erie, Huron, and Michigan much lower than usual; and in Lake Superior a rock at the entrance of Eagle Harbor appeared above water, and next year was 1¼ ft high, and in next year still higher. In 1846, Gull Island (a light-house station in Lake Ontario) re-appeared, after having been submerged 7 years. In January, 1847, sudden flux and reflux of Lake Ontario near Cobourg, when the waters receded 350 ft. and returned in an unbroken wave 4 ft. high; repeated 7 or 8 times till it gradually assumed its usual appearance. On 30th March, outlet of

Lake Erie temporarily blocked up with ice, so as to leave the Table Rock at Niagara Falls, and 200 ft. beyond it, dry. On 18th April, a sudden temporary depression of Lake Erie at Buffalo to 22 in. below zero, caused by a strong gale from the N. E. In 1851, *Lake Erie* at Port Colborne, was 3 ft. higher than in 1850; and in 1852 very little change; and in 1853 level nearly the same as in 1838 and 1830. In 1852 *Lake Ontario* 1 ft. 2 in. higher than in 1851; and in 1853, 9 in. higher, and calculated to be the same as in 1830 and 1838, and 4 ft. 5 in. above the minimum of 1849. In 1853, the River St. Lawrence generally considered as very high.

General Remark.—It is estimated that the Lakes subside irregularly, between the great periodical floods, at the rate of about 1 ft. 4 in. per annum; but that the comparative rapidity of the *fall* is as about 2 years, to 5 of the rise; and that the waters remain for some time at the mean level. Mr. Murray observes of Lake Huron, in his Report of 1848, that its waters have sunk considerably below former (perhaps ancient) levels, as indicated by water-marks, to the extent of 4 feet 10 inches.

While leaving the details of the foregoing Table to speak for themselves, I may be permitted to superadd, that the column of "*Comparative level*," however imperfect, is as complete as my materials would furnish; and that the greater part of the names there registered will be found among the different writers to whom I have had occasion to refer, or quote from, in the course of the foregoing remarks, besides several other sources of information to which I may hereafter have to advert; and further, that I have, under the head of "*Miscellaneous remarks*," taken care to refer to all such information of a loose *comparative* nature as appeared too indefinite for being admitted into the column of "*Authorities*," though not altogether to be rejected as without value; as well as to note not only any remarkable temporary derangements in the usual flow of Lake Erie, but any coincidence of level, or other remarkable event, connected with the state of the other Lakes at the same time. From a careful perusal of all details, I am disposed to think it will, in the first place, be satisfactorily demonstrated that not only there is no *regular* septennial or other great flood in Lake Erie, or any other of the great Lakes, but that, though in 1838 the whole of our inland waters happened to be simultaneously at an extraordinary height, it is very problematical whether they will always be found in an elevated or depressed state at the same time. For instance, taking it for granted that 1789–90 was really a maximum year, it will be seen by a reference to the Table, that instead of an interval of 14 years, the next maximum took place in 10 years, or in 1800–1; and that the next great flood was fortuitously in 1815; but that the next was in 1828; the next in 1838; and the last in 1853; and that, as might be reasonably expected, the advent of the mean and maximum periods was still less to be depended on. I am therefore bound to coincide in the more rational, and now generally received *opinion*, that the intervals at which these extraordinary floods occur are, at the best, uncertain, and mainly dependent on the extra amount of rain and snow, and the less degree of evaporation during the summer months, in that par-

ticular year; and that though the rise and fall in the different Lakes may, under ordinary circumstances, be generally simultaneous, it does not follow that such will always be the case; or in other words, that there may sometimes be a rise for a season, or part of a season, in one Lake, altogether independent of the others, arising from temporary obstructions at its outlet—a conclusion which I have arrived at, after much inquiry, observation, and reflection, in addition to the evidence furnished in the foregoing Table,—as will be found more particularly adverted to immediately.

2ndly. With regard to the *annual* variations in the level of the Lakes, and their general extent; and how far these also occur simultaneously, and are likewise owing to the amount of rain and snow compared with that of the evaporation; or what other cause:—I am free to confess that, *cæteris paribus*, and in accordance with the various authorities adduced, as well as all other information which I have been enabled to obtain, the same observation must apply to these variations as to the septennial fluctuations just noticed: but that while the extremes between the maximum and minimum range of the great floods may be rated at about six feet, the average difference of level during a single year may be between two and three feet; and that as already stated, though the rise and fall in all the Lakes may usually be simultaneous, one may sometimes be low while the others are high. As, for instance, it will be seen by a reference to the "Miscellaneous Remarks," that in 1795–96, Lake Ontario was so high as to drown trees of many years' growth while Lake Erie was described as so low that the gravelly beach near Cleveland was used as a public road; and that in 1814, "the upper Lakes were full," whereas "the centre and lower Lakes" were not so till the following year; and that in 1827 Lakes Erie and Ontario were between two and three feet above their usual level, while Lake Superior was lower than ever known before;—all which circumstances combined, with others yet to be noticed, have produced a conviction that each Lake is independently liable to irregularities of level peculiar to itself. I allude to the well known, but little thought of, *fact*, that during the winter months large boulders as well as smaller masses of stone and gravel, lying along shore, become firmly imbedded in the bordage ice, and on any rise of the waters, towards the close of the season, remain firmly attached to the moving floating masses, liable to be either dropt again in deep water, on the ice becoming what is called rotten, or to be removed to some distant part of the shore, if not to be carried along by the united wind and current towards the outlet of the Lake. Admitting such to be the case—for there is every year abundant evidence of the fact—it only remains to suppose that towards the end of winter, as frequently occurs, an accumu-

lation of loaded drift ice takes place near the head of the Rapids in the neighborhood of Buffalo, and becomes temporarily united by a fresh frost, and that a *jam*, as it is termed, then takes place, so as to leave a more contracted space than usual for the passage of the rushing volume of water below the broad roofing of ice, and that a further rise afterwards happens, coupled with a thaw, during which a deposition of the hitherto suspended rocky materials takes place at the bottom of the channel; it will then be evident that a still more contracted space will be left for the discharge of the increasing flood; and the natural consequence will be, that after the breaking up of the ice, the general surface of the Lake will have assumed a higher level than would have otherwise been the case, proportioned to the thickness of the stratum of boulders and other rocky materials deposited at the bottom of the channel, —liable to remain for a shorter or longer time, until gradually removed by the action of the sweeping current: a process altogether dependent on the strength of the latter, compared with the degree of compactness and solidification acquired by the rocky barrier opposed to it; and which may therefore require a whole season, or even more, to be accomplished. Of the motive power of ice, I myself have had ample proof, in the frequent dislodgment of boulders of large size from one part of the Lake shore to another, near my own farm; but more particularly of a vast rugged mass of limestone rock moved from comparatively deep water, some distance out in the Lake, to a more shallow part, so near the shore, that a large tree, dislodged from the high bank above by the undermining fury of the waves, happened to fall over in such a manner that its stem formed a very convenient though giddy bridge, from the beach to the stranger rock, and thereby allowed the latter to be afterwards used as a pleasant fishing station by my children. There are also, to my own knowledge, many instances of the removal of boulders in the different parts of the Rapids near Montreal. And among many examples of the almost entire temporary obstruction of the outlets of Lakes Huron and Erie by the jamming of the ice, I shall append to this paper an account of one which took place in the Niagara River, between Buffalo and Fort Erie, in March, 1848, with which I was at the time so much struck that I was induced to write to a friend on the spot for further particulars, in hopes of elucidating my long-cherished hypothesis; and such I have no doubt would have been the case had I been able to be present myself to compare facts. Independent of that, however, the particulars connected with the obstruction in the Niagara* alluded to, were of so extraordinary a character as to deserve being placed on permanent record.

* The account of this singular phenomenon is unavoidably postponed till some future time.

With respect to the 3d debatable question—the daily oscillations or other irregular transient tides observable in the different Lakes; I may observe that, making allowance for a greater or less degree of barometrical pressure, I might perhaps be disposed to assent in few words to the now generally received opinion, that in other respects they may be ascribed to the influence of the prevailing winds upon their broad expanse, more or less modified by their peculiar form and direction, and the relative bearing and nature of their extremities, as well as by the often very jagged and irregular outline of particular inlets or bays, and other inter-peninsular localities, such as Kewenaw Bay on Lake Superior, Green Bay on Lake Michigan, Presque Isle Peninsula and Long Point on Lake Erie, and the Bay of Quinté on Lake Ontario. But it seems to me that in so doing I would be conceding too much, as, in my humble unscientific apprehension, I am disposed to think that though such may be the case to a general extent, it is not the less necessary to prove, by a long and regular course of minute observations, whether such be the fact or not, as well as how far the surface of such vast bodies of water may not at times be considerably influenced by barometrical pressure on the one hand, or by lunar attraction on the other, particularly at the times of the vernal and autumnal equinoxes; and the more so, considering that late observations of philosophers in Europe have not only decided that there is a perceptible tide in the Mediterranean, Euxine, and Baltic, as well as in other altogether close saline seas, but also that something like barometric and lunar influence, or both, is observable on the inland fresh-water lakes of Switzerland and elsewhere. In confirmation of this I would, as regards the latter, beg to refer to the writings of Dr. Young, alluded to in an early part of these remarks, in conjunction with a valuable paper on the Lakes of Switzerland, by Colonel Jackson of the Royal Geographical Society, which lately appeared in the *Canadian Journal*, incorporated in a series of interesting articles on the variations in the level of the Canadian Lakes, from the pen of its learned editor, in which those oscillations (there termed *seiches*) are said to amount to no less than five feet. Nay, so interestingly appropriate to the present question do I regard a portion of the article alluded to, that I am tempted, in spite of the already great length of this paper, to transcribe the following, as the conclusion at which a learned German Professor has arrived on the subject:—"1st. That the *seiches* of the Lake of Geneva are much more frequent than is generally imagined. 2d. That they happen at all seasons of the year and at all hours of the day; but that they are generally most severe in the spring and in the autumn. 3d. That the state of the atmosphere seems to have a decided influence, it being remarked, that in proportion as that state is less changeable, so are the Seiches less frequent, and *vice*

versa. The Seiches have always been considerable when the atmosphere has been loaded with heavy clouds, or when the weather, in other respects severe, has threatened to be stormy, and when the barometer has sunk. 4th. That though Seiches are more frequent in spring and autumn, they are more considerable in the summer, and, in particular, towards the close of the season. The highest that have been observed happened in the month of September. 5th. That the minimum of the Seiches has no precise term: their maximum seems to be five feet. 6th. That although the duration of the Seiches is very variable, the greatest extent seems not to exceed 20 or 25 minutes, but usually lasts a much shorter time. And 7th, That they are not peculiar to the Lake of Geneva alone; M. Vaucher having observed them on the Lakes of Zurich, of Annecy, and of Constance."

I cannot refrain from also quoting the following paragraph from the same paper, as much to the point:—

"It appears unquestionable that the phenomenon of the Seiches is due to an unequal pressure of the atmosphere in different parts of the Lake at the same time, *i. e.*, to the simultaneous effects of columns of air of different weight, or different elasticity, arising from temporary variations of temperature, or from mechanical causes; and if such be the case, all Lakes of a certain extent, and even inland seas, must be subject to the same influence, and therefore present the same phenomenon; and I have little doubt that correct observations will verify the presumption."*

With respect to the irregular tides observable in the Baltic and Black Seas, and other great bodies of saline water of a similar character, it will be sufficient to give the following, regarding the first-named sea, from a standard geographical work, as bearing intimately on the subject under discussion:—"The Baltic being a close sea, is of course not subject to the phenomena of regular tides. But though such be wanting, a variation in the height of its waters, equal, frequently, to 3½ feet Swedish, is observed at irregular intervals. This occurs at all seasons, but chiefly in the autumn or winter, at the time of heavy rains, or when the atmosphere is charged with clouds, though unattended with falling weather. The water maintains its height frequently for several days, sometimes even for weeks. Prevalent winds, flooding rains, melting snows, and many other causes were assigned for this very remarkable phenomenon; but it continued to occur, independent of all these, till 1804, when Schulten, a Swedish physician, after having collected all the observations that had been made, found 'that the greatest height of the water corresponds with the greatest depression of the barometrical column; and conversely.' The almost total absence of oceanic action in this

* See Canadian Journal, vol. ii, pp. 27, 28, &c.

sea leaves the cause thus assigned to operate with full power; and if Schulten's hypothesis be confirmed, of which there is now but little doubt, it will, in all probability, serve to explain similar phenomena observed in other close waters, as the Caspian, Lake Balkai, and the Lake of Geneva, to which Saussure has assigned similar causes."*

[An important paper on the fluctuations in the surfaces of the Lakes not referred to by Major Lachlan is published by Charles Whittlesey in Foster and Whitney's Report on the Lake Superior Land District, Part 2, 1851, p. 319. It reviews the facts, adds a large number of observations and sustains the conclusion that the rise is not periodical.—Eds.]

ART. V.—*Remarks on the changes which take place in the Structure and Composition of Mineral Veins near the surface, with particular reference to the East Tennessee Copper Mines;* by J. D. WHITNEY.

IN the number of the American Journal of Science for March last, ([2] xix, 181,) M. Tuomey has given "a brief notice of some facts connected with the Ducktown, Tennessee, Copper Mines." As this notice seems to me to convey a wrong impression with regard to the mode of occurrence of the ore in the veins of the region alluded to, I take the liberty of stating what I conceive to be the correct interpretation of the phenomena displayed on so large a scale in the Polk County, or Ducktown Mines.

Mr. Tuomey remarks as follows: "in every published account of the mines that I have seen, the impression is left, *that the ore (b) is derived from the underlying portion of the bed (c) by decomposition.*" No such impression could, by any possibility, be derived from the descriptions of the East Tennessee Mining region which I have published. To show this, I quote from the report† to which Mr. Tuomey refers, at the commencement of his notice, as follows: "On penetrating beneath the surface, the section represented in the annexed figure is obtained. (See section accompanying Mr. Tuomey's article.) Beneath the gossan, is found a bed or mass of black cupriferous ore, of variable thickness and width. This, as well as the gossan, is the result of the decomposition of an ore consisting originally of a mixture of the sulphurets of iron and copper, which was associated with a quartzose gangue or vein-stone. The place of the bed of copper ore marks the limit of the decomposition of the vein; *beneath it the*

* See McCulloch's Geographical Dictionary, Article *Baltic.*

† Report on the Mining tract of the East Tennessee and Cherokee Copper Mining Co.; by J. D. Whitney. New York, 1853.

ore exists in its original condition." It seems hardly possible that any one should ever have supposed the black ore to have resulted from the decomposition of any other part of the vein than that which lies above it, as below it everything remains in its original, unaltered condition.

The decomposition of metalliferous lodes in their superficial portions is a matter often noticed and generally expected by the miner, and there is nothing anomalous in this respect in the East Tennessee Copper region. The commonly observed facts are these: the predominating metalliferous ores which are wrought in mines, especially of silver, copper and lead, are sulphurets, sulphur being the usual mineraliser, although arsenic and antimony are not unfrequently found in connection with sulphur in combination with these metals. These ores are sometimes scattered irregularly through the gangue in fine particles and sometimes arranged in nearly parallel bands or plates, which are separated from each other by belts of barren veinstone. This is the normal condition of the veins at a considerable depth and some of them retain their original appearance and remain chemically and mechanically unchanged up to the very surface. In most metalliferous lodes, however, it is found that the ores have undergone decomposition down to a certain depth, which rarely exceeds 300 feet and generally falls between 50 and 100 feet. This decomposition is perhaps more common and more strongly marked in cupriferous lodes than in those of the other metals, although some argentiferous veins in South America exhibit it on a grand scale. The predominating ores of copper are the variegated ore and copper pyrites, both of which are combinations of sulphur with copper and iron and their presence in the veins beneath is indicated on the surface by an outcrop of what the Cornish miners call *gossan*, a term which has been generally adopted wherever the English language is spoken. This is a hydrated peroxyd of iron, usually much mixed with silicious and earthy matter and having a somewhat open and porous structure. Associated with this ferruginous mass the oxydised combinations of copper are often found occurring, at no great distance from the surface; among these, the carbonate and silicates are the most common, the phosphate and arseniate less so. The oxyds themselves and the native metal are among the products of decomposition. Sometimes these oxydised ores are very abundant in the upper part of a cupriferous lode, and are wrought with large profits, owing to their richness and the softness of the ground and the consequent facility in mining. In other localities, nearly all the copper has disappeared from the upper portion of the vein and only traces of these ores are found with the gossan. On sinking down into such decomposed veins a gradual change is found to take place in their character: the oxydised ores are replaced by the sulphurets; the

ferruginous aspect of the lode disappears; the gangue becomes more solid, and the walls are better defined.

These changes in the upper portion of the sulphuret-bearing lodes are usually conceived to be the result of the action of air and water introduced from the surface and penetrating gradually downwards. Through their joint influence the sulphuret of copper and iron is gradually decomposed and while the latter metal remains behind in the form of an impure hydrous oxyd, or gossan, the copper is also converted into an oxyd and may remain in that state, or combine with the sulphuric acid furnished by the oxydatiou of the sulphur of the original ore, or with any other acid which may chance to be present, thus giving rise to the numerous beautiful ores, most of which contain water, which are so common in the higher portion of cupriferous veins. The nature of the combinations resulting from any such decomposition and their relative quantity must, of course, depend on the quantity and quality of the ore originally in the lode, the proportion and kind of vein-stone and probably still more on chemical and perhaps electric agencies, the precise mode of action of which is as yet but imperfectly understood.

In the Polk County mines, these changes are displayed on a grand scale. The metalliferous veins which belong to the segregated class, are very wide and the decomposition has been very complete, so that the outcrop of gossan is very marked and in some places occupies a width of 100 feet on the surface, consisting of large angular blocks of ferruginous rock piled up along the line of the vein. On sinking into this mass of ferruginous matter it is found to be tolerably soft, but at the same time so compact that excavations in it need but little timbering. If the shaft is commenced on the summit of a hill, it will be necessary to penetrate a hundréd feet, perhaps, before any change in the nature of the vein is perceived. In the valleys the distance required for this purpose is much less. The depth at which the gossan terminates is nearly coincident with the water-level, or the point where, in sinking, water is found in considerable quantity. Here a layer or bed of copper ore is met with of very irregular dimensions, in some places occupying large bunches or pockets of many cubic yards in content, and in others forming only a thin stratum. This deposit of ore is quite as variable in composition as it is in dimensions. Its color is usually quite dark, and when rich in copper, almost black. It is evidently a mechanical mixture of black oxyd of copper with sulphurets of iron and copper, sulphate of copper, oxyd of iron, silicious matter, and some manganese. The per-centage yield of copper is usually low; but the purest portions contain from twenty to thirty per cent. of metal. This deposit of black ore is the object of exploration in the mines, and the only source, thus far, from which copper has been obtained in any quantity worthy of notice.

Beneath the black ore is the undecomposed portion of the vein, showing, in two or three points, where I was able to see it at the time of my visit (1853), a hard quartzose gangue with particles of copper pyrites scattered through it, and associated with a considerably larger quantity of iron pyrites. There seems no reason to suppose that the ore which originally existed in the upper part of the vein, from whose decomposition the black ore was derived, was any different in nature from that found below, although there may have been bunches of it considerably richer in copper. The deposit of black ore is insignificant in dimensions, compared with the mass of gossan which overlies it, and when we consider that a large portion of the copper which was once disseminated through perhaps a hundred feet of overlying vein-stone is now concentrated into the thickness of perhaps two or three feet, on an average, it will be seen that it is not necessary to suppose that the whole of that portion of the vein which is above the bed of black ore, "doubtless once consisted of yellow sulphuret of copper," as Mr. Tuomey supposes to have been the case. Certainly there is no reason to believe that the black ore is a sulphuret of copper altered by heat. Apart from the consideration that it is not such a product, or mixture of products, as would be produced by any igneous action on copper pyrites, we can conceive of no way in which the effect of increased temperature could be limited to the upper portion of the vein, so that that only should undergo decomposition. That the changes in question are exclusively the result of a humid process, can, as it seems to me, be hardly doubted. The concentration of the black ore in one stratum seems to have been due to the percolation of the surface water which was constantly carrying it downwards to the point where it was stopped by the solid portion of the vein.

That the subject of the decomposition of veins is one which is thoroughly understood should by no means be inferred from the preceding remarks: there is, on the contrary, much in these phenomena which has not, as yet, been satisfactorily explained. We know, indeed, that the changes of the sulphurets with oxydised combinations do occur, for we see them taking place under our own eyes, through the joint action of air and water holding carbonic acid in solution; but why in some mining districts the metalliferous veins should be thus effected, while in others no change whatever has occurred, is less easily understood. Burat has called attention to this circumstance, and cited some instances in which the sulphurets remain entirely unoxydised up to the very surface. Thus the cupriferous veins of Mouzaia in Algiers project out from the surface like walls, being more permanent than the adjacent rock and the first blow of the hammer reveals the pyritiferous ore in its natural state. The same thing may be observed in this country. Throughout the Northern states the pyritiferous lodes remain apparently in their unaltered condition;

or, at most, have undergone but little change and exhibit hardly any indications of gossan. The enclosing rocks are not at all softened or stained with ferruginous matter. As examples of this he would instance the great veins of Shelburne, and Eaton, in New Hampshire; those of Hampshire County in Massachusetts; and the St. Lawrence County mines in New York. In none of these has any marked change taken place near the surface. In one part of the Southampton (Mass.) lode a few oxydised ores were found when the mine was first opened, but they were but small in quantity compared with the mass of the unaltered ore. This state of things is a great drawback on the opening of the New England mines since the expense of sinking and driving in the hard granite and quartzose rocks is enormous. In North Carolina, South Carolina and Georgia, on the other hand, the gneiss and slates are often found over a great extent of territory completely decomposed and softened, so that they may be excavated with the pick and shovel, down to a depth of fifty or a hundred feet. I have known a shaft sunk in North Carolina in the rock to the depth of sixty feet in one week.

In the veins of that State, the principal, indeed, almost the only, one near the surface is an auriferous gossan resulting from the decomposition of iron pyrites, with which a little copper pyrites occurs intermixed. Of this latter ore, the quality in several instances seems to increase with the depth of the workings. If the veinstone is wholly quartzose the extent of the decomposition is much less than when it contains feldspathic or slaty portions. Thus in the McCullock mine, in Guildford County, N. C., there is a body of soft ferruginous ores containing a good amount of gold which extends downwards more than 100 feet and parallel and coëxtensive with this auriferous mass, which may be mixed with a shovel, there is a heavy bed of quartz with iron and copper pyrites scattered through it, in which no traces of decomposition can be perceived.

With regard to the East Tennessee veins the practical question of the most importance is: what kind of ore and how much of it is likely to be found in sinking into the undecomposed veins beneath the level of the black ore. This, we believe, can only be determined by actual trial. If in cleaning out the deposit of ore, which lies upon the hard veinstone beneath, there should be bunches of cupriferous ore found, the best of them should be opened by sinking on them, and there can be no satisfactory reason given, based either on analogy or on the appearances of the bodies themselves, why considerable quantities of the yellow ore of copper should not be found within a reasonable depth. Still it is not impossible, that, as these views do not exhibit the characteristics of true fissure veins, they may be found to have been richest near the surface and not to be capable of being worked with profit in the hard rock.

Art. VI.—*On a Universal Indicator for Microscopes*; by Prof. J. W. Bailey, U. S. Military Academy.—With a Plate.

In the Quarterly Journal of Microscopical Science, vol. i, p. 34, an ingenious contrivance for registering the position of microscopic objects is described by Mr. Tyrrel; a modification of this, by Mr. Aymot is given in a subsequent number (l. c., vol. i, p. 301); and a still better arrangement for the same purpose, suggested by Mr. Brodie and applied by Mr. Okeden to his microscope, is described at p. 166 of volume iii. of the same work. The last mentioned device can scarcely be improved upon for convenience; but there is one defect which is inherent to all these inventions, viz: that they are essentially selfish contrivances, of no use to any one but the owner of the particular instrument to which they may be attached.

The object of the instrument I propose is more comprehensive than that of the "Finders" above alluded to; being no less than to make a Universal Indicator by means of which an observer can so register the position of any number of objects mounted upon slides, that when these are sent to a distant correspondent the latter may be able by means of the Indicator to find at will any of these objects as easily as if he had the identical microscope and "Finder" by which they were at first recorded. If such a mode of recording the position of objects can be generally adopted that when the register is once made, the record and the objects shall then be entirely independent of the original instrument and observer, and applicable to any microscope, it will tend to promote science not only by facilitating the interchange of specimens among naturalists, but it will give to each observer's collection when properly registered, a permanent scientific value and utility which it could have in no other manner.

The plan I have adopted is to make upon an engraved card what may be considered as a transferable stage having guide lines by means of which the centre of the field of view of the microscope, and the position of a slide when any object upon it occupies this centre, may be given.

Plate I. shows the Indicator complete. The centre of the field of view corresponds to the intersection of the horizontal line C, D, with the vertical line E, F. On the right and left hand of this centre the vertical axes B, and A′, are placed at distances of $\frac{4}{5}$ths of an inch, and the axes A, and B′ are similarly placed at the distances of $\frac{6}{5}$ths of an inch from the centre.

The axes are then graduated as seen in the plate; the small divisions being each $\frac{1}{50}$th of the standard inch.

The dotted lines G, H, I, give the outline of what will be referred to as the *centre piece.*

Should it ever be desired to reproduce the Indicator by engraving or otherwise, the dimensions above given must be most accurately preserved. The dimensions here given were taken from the standard inch of the United States belonging to the State of New York and preserved in the office of the Superintendent of Weights and Measures in Albany. It is the same as the English inch.

The slides on which objects are mounted to be used with the Indicator must have *guide-lines* ruled on their *under side* as shown in fig. 1 and 2. The horizontal line parallel to the lower edge and passing through the middle of the slide is not continued over the portion of the slide which is to be occupied by the objects and their glass cover. The distance of each of the vertical lines from the middle point of the slide is *one inch*. Great accuracy in the distance between these lines of the slide is not essential when they are to be used with the ordinary form of the Indicator as above given, but it is desirable when they are to be employed as hereafter described with a modification of the Indicator applied to a moveable stage.

The slides should all be marked with an arrow placed upon their upper and right hand corner, as shown in fig. 1 and 2, to point out the edge which must always be kept in front in using the slides upon the Indicator.

The Indicator is to be used as follows:—Cut out the centre piece with a thin bladed knife following the outline G, H, I; then replace the piece cut out, and make a hinge for it along the line G, H, by pasting underneath it a piece of thin paper which will bear repeated folding, so as to connect it to the rest of the card.

The Indicator being now ready for use, it must be firmly secured to the stage of the microscope in such a position that its centre as given by the intersection of the lines C, D, and E, F, when viewed as an opaque object, may be exactly in the centre of the field of view. If the stage is a moveable one, it must be kept stationary after the Indicator is properly centred.

The Indicator having been adjusted as above directed, the centre piece is to be turned down and the instrument is then ready for use, either to record new objects, or to find those previously recorded. The slide is to be put upon the Indicator, and guided either by the fingers or a moveable ruler so that when any object which is to be registered occupies the centre of the field of the microscope, the horizontal guide line upon the slides shall pass through the same numbers on two vertical axes of the Indicator as remote from each other as possible. In some positions of the slide the axes A, and B′ can be used for this purpose; in others A, and A′, or B, and B′ must be employed.

The horizontal line of the slide being arranged as just directed it will be found that at least one of the vertical guide lines of the

slide will intersect the horizontal graduation. By observing now the numbers at which the guide lines respectively stand, the record can be made. Suppose for example that the horizontal guide line ruled upon the slide intersects the verticals of the Indicator at 43, while the right hand vertical of the slide cuts the horizontal series of numbers of the Indicator at 75; the entry to be made for this object in the register would be written thus $\frac{4}{7}\frac{3'}{5}$ and whenever that particular object is to be found either by the same Indicator or *any other copy of it*, if the slide is placed at these numbers and the Indicator is properly centred, the object must be in the field of view. In the same manner any number of objects can be registered or found. If the slide happens to be so placed that both of its verticals intersect the graduated portions of the horizontal line C, D, the position of either one of them can be recorded at will.

If a guide line upon the slide falls between two divisions of either scale, the fraction of the division may be estimated with sufficient accuracy by the eye or a hand magnifier and entered in the register. Thus the recorded position $\frac{25\frac{1}{8}'}{34\frac{1}{4}'}$ would mean that the vertical lines of the Indicator were intersected at $\frac{1}{8}$th of a division of the scale beyond 25, while the vertical guide line of the slide passed $\frac{1}{4}$th of a division beyond the number 34 of the horizontal scale, as nearly as could be estimated.

It is convenient to let the lower edge of the glass slide rest against a straight edged guide piece which can be moved parallel to the horizontal line of the Indicator. By pushing the slide along this edge, all the objects on the same horizontal can be found without changing the position of the guide piece. By moving the guide piece a little forward or back another sweep across the slide may be made, and so on until every object of interest is found.

By following the directions above given it will be found that the recording or finding of objects by means of the Indicator is very easily performed, and scarcely requires the time which has been employed in describing the method. It is believed that the explanation above given is sufficiently explicit to enable any one to use the Indicator; but some additional remarks will now be made upon the reasons for adopting the particular arrangement I have used, the modes of insuring accuracy in manipulation, and the modifications of which the Indicator is susceptible for individual convenience.

It was desired to make the instrument capable of universal application; so simple that it could be adapted to any stage; so light and yet so strong that it could be sent without injury by mail or otherwise to any distance; and lastly that the different copies should be perfect fac-similes of each other and reproducible

at any time. All this is secured by having the Indicator engraved upon a steel plate and printed upon cards of uniform quality, and by taking the dimensions from the standard United States inch, preserved in the office of the Superintendent of Weights and Measures in Albany. In order to extend the use of the Indicator to all cases which are likely to occur, the graduation was arranged with reference to slides three inches long and one inch wide, while it will answer equally well for smaller ones. When these slides are not covered with paper, and guide lines can be ruled as above directed upon the glass itself, the graduations necessary for their use would only extend upon the verticals ½ an inch above and below the horizontal line, and upon the horizontal line only ½ an inch outwards from the points 40 and 70; but in order to provide for paper covered, or opaque slides whose uppermost and lateral edges may be used as guide lines, the graduation has been extended considerably. It will be found on trial that slides of the standard size, whether paper covered or not, may be employed with the Indicator for the registration of all objects under a glass cover of a square inch in size, which is quite as large as is likely to be used. In using covering glasses of an inch square it will be found necessary to employ the horizontal numbers 0 to 50, and the verticals A, A′, for objects towards the right of the cover, and the other axes and numbers for objects towards the left. For a portion of the objects under the cover either set of axes and numbers may be used at pleasure provided that the verticals are chosen as far apart as possible.

Two verticals on the same side of the centre should never be used together, as a small error in observing the numbers would have more effect in displacing the object from the centre than if two axes at a greater distance had been employed. The reason for leaving a blank ungraduated space between 50 and 60 on the horizontal line was to allow a fac-simile of the Indicator to be engraved upon the stage of any microscope, the blank space being left for the portion of the stage occupied by the aperture.

The guide lines upon the glass may be ruled with a fine pointed scratching diamond, and be rendered more visible by having graphite or black lead rubbed into them. Lines ruled in this manner will answer for all except very minute objects, but in consequence of the widening of the lines by the chipping up of the glass due to unequal expansions and contractions, the lines often become too wide and irregular for very accurate adjustments. In such cases admirable guide lines may be etched upon the glass with the *vapor* of fluohydric acid and can be made of any required degree of fineness. The *solution* of the acid should not be employed for the etching as it gives lines which are too smooth and difficult to see, and which will not retain the black lead if rubbed into them.

The power of the objective employed in determining the position of an object for registration, should always be the highest which can be conveniently employed; while in searching for an object already recorded, a power lower than that employed in the registration may be used. The object then must be in the field of view, and would be at the centre but for slight errors in manipulation, or the want of perfect adjustment in the mountings of the object-glass. Care should be taken to bring each object accurately to the centre of the field of view, before recording it. It will then require an error equal to half the diameter of the field of view to throw it out of that field. For example the field of view of my $\frac{1}{2}$ inch objective made by Spencer, includes two divisions of the Indicator, and hence an error of nearly one division might be made in placing a slide upon the Indicator by means of its recorded numbers and yet the object would be found in the field.

It may happen that in transferring a slide from one Indicator to another that the object when brought into sight by means of its recorded co-ordinates will not appear well centred. If this be due to slight differences in determining the centres of the Indicator, and yet the record has been carefully made, it is easy to correct for the difference in the following manner. Move the Indicator *with* the slide placed at any recorded position until the object comes into the centre of the field of view, then secure the Indicator to the stage in this new position, and all other objects recorded by the same Indicator ought to be brought to the centre of the field of view by means of the numbers as registered.

The convenience of the Indicator for individual use may be increased by several slight changes. One of these consists in removing the paper centre piece, and replacing it either temporarily or permanently by a glass plate bearing lines at right angles to each other ruled very lightly with a diamond point, and so adjusted as to coincide with the prolongation of C D and E F through the centre. For all but the highest powers there is no objection to having these excessively minute lines permanently beneath the centre of the Indicator as they do not perceptibly interfere with the light, and it is convenient to have them always in place. They can be ruled upon a piece of mica or thin glass cemented to the back of the Indicator, or the latter may itself be cemented to a piece of plate glass and the central guide lines then carefully ruled. Even for the highest powers these lines can be used in recording the position of objects, which can then be found for study by using an Indicator of the ordinary form. By a proper arrangement, a moveable stage, with screws for vertical and horizontal motions, may be graduated so as to correspond to the Indicator, and yet preserve all the advantages of accurate adjustment which the screws afford. For this purpose it is necessary to ob-

serve that if the Indicator be placed upon the stage and accurately centred, with its guide line C, D, parallel to the front edge of the stage, and a slide be then placed upon the Indicator so that its horizontal guide line shall coincide with C, D, and the right hand vertical guide line stand at 70, (*i. e.* in the position which would be recorded as $\frac{5}{7}\frac{0}{0}'$,) or its left hand guide line at $\frac{5}{4}\frac{0}{0}'$; then a motion of the stage itself bearing with it the Indicator and slide, or an equal motion of the slide upon the Indicator and fixed stage will bring the same point of the slide to the centre of the field of view.

Therefore by attaching to the stage in any convenient manner graduations corresponding to those of the Indicator, and by having lines corresponding to $\frac{5}{7}\frac{0}{0}'$ and $\frac{5}{4}\frac{0}{0}'$ ruled upon the stage, it will only be necessary to place the slide directly on the stage at these numbers, the stage itself being set either at $\frac{5}{7}\frac{0}{0}'$ or $\frac{5}{4}\frac{0}{0}'$ of its graduations. By turning the milled heads of the screws which give the vertical and horizontal motions of the stage, the object can be brought into the field of view and recorded or found again by means of the numbers attached to the stage; while the record may be used for any other Indicator as if made in the usual manner. If the distance between the guide lines upon the slide agrees accurately with that between 40 and 70 of the Indicator, the slide when placed upon the moveable stage at either $\frac{5}{4}\frac{0}{0}'$ or $\frac{5}{7}\frac{0}{0}'$ will need no displacement for the whole series of numbers; but if this distance does not agree, the slide must be put with its left hand vertical coinciding with the left hand vertical of the stage for all numbers from 0 to 50 of the horizontal series; while from 60 to 110 of the same series the slide must be set so that its right hand vertical coincides with the right hand vertical of the stage; in each case the horizontal lines of the stage and slide being adjusted to coincide. By observing this rule the necessity of perfect accuracy in the position of the guide lines upon the slides is done away with.

There are some objections, but not insuperable ones, to the moveable stage Indicator as above described. In the first place the stage as usually made has its motion too limited to correspond to the whole range of the Indicator, and secondly the guide lines ruled upon the stage for one object-glass may not answer for other powers on account of slight inaccuracies of mounting.

The stages can doubtless be constructed to give as wide a range for motion as required, which will do away with the first mentioned objection. The second may be removed by placing an Indicator upon the upper plate of the stage when the latter stands at $\frac{5}{7}\frac{0}{0}'$ and adjusting it so that when well centred *for the power employed* the line C F shall be parallel to the front edge of the stage. The slide being then placed upon the Indicator with its guide lines at $\frac{5}{4}\frac{0}{0}'$ or $\frac{5}{7}\frac{0}{0}'$ the remaining motions may be made with

the screws in the usual manner and the numbers may be read off from the stage scales instead of the Indicator.

The above mentioned modifications are excellent for individual convenience; but for the general purposes of science, the comparable, transferable, reproducible Indicator, in its simplest form, must be preserved; and it is only in that form that it deserves the name, suggested by a friend, of the Universal Indicator.

As a proof of the utility and accuracy of the Indicator, and of its convenience as a means of scientific exchange, I may state that numerous mounted slides of minute recent and fossil Diatoms have been exchanged through the Post Office by Judge A. S. Johnson of Albany, and myself, and that each has found by the ordinary as well as modified forms of the Indicator all the shells however minute, fragmentary or previously unknown, which the other had recorded. Some of these objects were less than a $\frac{1}{1000}$th of an inch in diameter, and yet they were found without difficulty by means of the Indicator.

To determine whether different impressions of the Indicator when made on the same kind of paper were comparable, a set of objects was registered successively by seven different impressions made on enamelled cards some of which were arranged with the ordinary paper centre piece, and others with the central guide lines ruled upon glass. The numbers being recorded for the objects when well centred upon one of these Indicators, the slide was then transferred to each of the other Indicators and each object being brought into the field by its recorded numbers, the position was carefully adjusted so that the object should be well centred, and a record for each copy of the Indicator was thus made. On comparing the different numbers it was found that the coincidence was almost perfect, the difference never exceeding $\frac{1}{4}$ of one of the divisions of the Indicator, an amount which might be quadrupled before an object would be thrown out of the field of view of my $\frac{1}{2}$ inch objective.

The Indicator having been put to so many and such severe tests I feel no hesitation in recommending it as a means of scientific intercourse among observers, and as a means by which collections of microscopic objects may be registered, arranged, and catalogued; and an index to the whole so made that any particular specimen may be found at will either by the original observer or any one into whose hands the slides and accompanying register may at any time come.

The copy of the Indicator which accompanies this paper is not given for use with the microscope, as the kind of paper upon which it is printed is different from that used for the standard Indicator, and therefore in consequence of unequal shrinkage a slight deviation is produced. The Indicator for use with the microscope

is printed upon enamelled cards, and the different impressions have been found to agree so closely with each other as well as with the original plate that no appreciable error is perceived.

I can not close this paper without expressing my warm thanks to Judge A. S. Johnson of the New York Court of Appeals, for his cordial sympathy and aid in testing the merits of the Indicator and for some excellent suggestions as to its best form for general use. I should also express my obligations to the engraver, J. E Gavit, Esq, of Albany, who has spared no pains in making the steel plate from which the Indicator is printed as accurate as possible.

Art. VII.—*On the Composition of Eggs in the animal series;* by Valenciennes and Frémy.—Part III.*

Eggs of Reptiles.

In the two preceding memoirs, we have given the results of our observations on the eggs of Fishes. We have pointed out in these eggs the presence of the proximate principles, ichthin in Rays and Sharks, ichthulin in the species of the numerous families of osseous fishes, and have directed attention to the important fact, that this principle is gradually modified in proportion as the egg approaches its maturity, and that it ends by disappearing in the egg when it is ready for fecundation, the ichthulin being then replaced by albumen.

We now continue the account of our researches on the eggs of other oviparous animals.

Eggs of Tortoises —We have examined the eggs of two species of Chelonian reptiles belonging to two distinct genera. Some were hatched by a land Tortoise from Algiers, which M. Dumèril has called *Testudo Mauritanica.* This female has lived for many years in France, at the house of a resident of Grandville. The second is the fresh-water Tortoise of Europe, the mud Tortoise (la bourbeuse) of Daubenton and Lacépède, which M. Dumèril calls *Cistudo Europæa.* Although these two species of Chelonians inhabit different countries and climates, the resemblance in the constitution and composition of the liquids of their eggs is well worth remarking; but there is a further similarity which we hardly expected; it is that they have a great analogy to those of cartilaginous fishes. They are formed of a whitish gelatine, not very abundant, hardly albuminous, shut up in the cells of large transparent membranes. The yellow very rich in albumen, contains besides a considerable quantity of phosphuretted oil, and also peculiar grains constituting an entirely new principle. These eggs do not contain a material precipitated by water analogous

* From the Journal de Pharmacie for August, 1854: translated by Dr. Rosengarten. For Parts I and II, see last volume, p. 38, 238.

to ichthulin. On treating their yolk with water, the yellowish oil of the egg is seen swimming on the surface of the liquid, and is precipitated in little white grains. The water retains in solution albumen and the salts. The little grains can be easily purified by washing in water, alcohol and ether. By these processes a material is obtained which presents, when in great purity, a certain analogy to ichthin, but which differs by characteristics that prevent its being confounded with the substances from the eggs of fishes. We have given to this new substance the name of Emydin.

Emydin.—The grains of this substance are rounded or a little ovoid, and covered with little roughnesses. They are white, transparent, harder and denser than the grains of ichthin. The largest grains of emydin taken from the eggs of the Mauritanian Tortoise (Testudo Mauritanica) are 10 hundredths of a millimeter. They appear enveloped in a very thin membrane, which has the appearance of tearing when compressed under the microscope, or which shows, by its transparency, a sort of nucleus frequently of a hexagonal form. This nucleus increases gradually, its angles disappear, and it ends by becoming spherical. Other granules break as they expand or swell out. They are of all sizes, from one hundredth of a millimeter to very large dimensions. The grains of the *Cistudo Europea* Dum., are smaller than those of the land Tortoise, for the largest did not exceed 6 hundredths of a millimeter, and they appear in general, to be more spherical than those of the other species. Although resisting under the pestle, we have broken them, and found that they break into spherical fragments, from the circumference to the centre. Potash, well weakened, immediately dissolves emydin, while it acts quite feebly on ichthin. Acetic acid, which, as we know, dissolves ichthin with the greatest ease, simply swells the grains of emydin, but does not dissolve them. These characteristics seem to establish very clear differences between ichthin and emydin. Its grains are dissolved in boiling hydrochloric acid, without giving to the liquid any violet color; this action proves that emydin is not the vitellin of bird's eggs.

Emydin submitted to analysis, shows the following composition:—

	Proportion of azote.		Per-centages.	
Solid matter,	0·313	Solid matter, 0·370	Carbon, . .	49·4
Water,	0·210	Azote, 0·0579	Hydrogen, . .	7·4
Carbonic acid,	0·568		Azote, . .	15·6
			Oxygen and phosphorus,	2·67

This substance appears to us to be isomeric with ichthin. The grains of emydin leave, on burning, a residue of calcareous salts which never exceed 1 per cent. We have also established that the eggs of our Testudo Mauritanica from the same laying present remarkable differences. The yellow of certain eggs did

not contain grains of emydin ; but in that case, the white, in place of being transparent and colorless, had a slightly yellowish tinge. There were visible in it, under the microscope, little grains of irregular form which seemed to have some analogy to the grains of emydin of the vitellus.

Lizard's Eggs.—According to our observations on the eggs of the Lacerta "vert piquetè," and on those of the Lacerta stirpium (Lezard des souches), their vitellus presents a certain resemblance in composition to that of the yolk of birds. We found neither grains of ichthin nor emydin.

Adder's Eggs.—We have examined eggs of the Ringed adder (*Conleuvre à collier*), and those of the Esculapian adder. Our analyses confirm those of MM. Martin-Saint-Ange and Baudrimont. We found the vitellus of these eggs scarcely surrounded by a very thin layer of albumen. The yellow is formed of albumen and of phosphuretted fat, and it appears to throw down vitellin when washed with water.

Viper's Eggs.—The eggs of the common viper differ in several ways from those of the Adder. They contain a white, like that of the Ray, of a gelatinous consistency ; it is formed by elastic hyaloid membranes, with a slightly albuminous fluid. The yellow of the Viper's egg, presents itself, like that of the adder, in the shape of a very albuminous liquid, holding suspended in it a considerable quantity of fat ; when this yellow is put into contact with water, it produces a phenomenon that we have remarked in no other species of eggs. The liquid, at first quite fluid, thickens gradually, and soon becomes completely gelatinous. This change of state in the liquor is due to a body like vitellin, found at first in solution in the albumen, which becomes insoluble under the slow action of the water, and finishes by making it harden. Is this vitellin, or something different? We were not able last year to resolve this interesting question ; we hope to do it, if we are fortunate enough to get a sufficient quantity of Viper's eggs. We shall also during the present season continue to study the eggs of the Sauria and the Ophidia.

The Eggs of Batrachia.—The numerous observations made upon the eggs of Batrachia have not yet established the nature of the substances which constitute them. The gelatinous matter of the eggs of the Frog is formed of a tissue of hyaloid membranes, containing water absorbed after hatching. It is modified by cooking, becoming slightly opaque, and the vitellus hardens, which proves the existence of albumen. The vitellus blackened by a black pigment peculiar to these eggs and coloring the membrane, which is excessively thin but may be recognised by its dryness, contains in its liquid, a very small quantity of fatty matter united in transparent drops, a matter which looks under the microscope like a black punctation of extreme fineness, and then an

abundance of vitellin granules, very small, transparent, varying in form, more commonly rounded, insoluble in water and soluble in acetic acid. We repeated this experiment several times before being certain of its character. The eggs of the *Tritons* show a like composition. The exterior white is like that of the Frog's egg. The citron or green vitellus, according to the species, contains fat and a considerable number of vitellin granules quite rounded, which we have studied in the egg taken in the oviduct near being hatched, and in the ovula still enclosed in the ovary. The examination of these last has shown us this remarkable fact, that the vitellin granules increase with the age of their formation. They are very much smaller in the ovula than in the vitellus of the egg. These granules are also insoluble in water and soluble in acetic acid. The egg becomes hard when cooked in boiling water; it then contains albumen too, as well as that of the frog. We have already mentioned that MM. Martin-Saint-Ange and Baudrimont saw the granules of the Frog; they have even given their figure. M. Rud. Virchow has also given some accurate observations on the granules of Frog's eggs, Toads and Tritons. (Zeitschrift für Wissenschaftliche Zoologie, par MM. Siebold and Kölliker, vol. iv, part 2. p. 236; 1852.)

The characteristics which we describe, lead us to the conclusion, that the granules contained in these eggs are of the nature of ichthin, that is, that they are of a like nature with those of the Ray's and the Shark's eggs. The simple view of the grains under the lens of a microscope suggested this idea; their characteristics confirm the identity, which has brought us to the establishment of this curious and important physiological fact, that the Batrachians besides undergoing, in consequence of their metamorphosis, a primary condition of existence like that of fish, lay eggs whose composition has the greatest affinity to those of fish.

This similarity holds even in the ovules, for we have already remarked that the granules of the ichthin from a Ray are smaller in the ovules than in the yolk of the egg of these fish. We there show, in fact, a like composition for the white surrounding the vitellus, and the presence of ichthin, that immediate new principle abundant in the egg of cartilaginous fishes.

M. Virchow whom we quote, has likewise observed the granules (Dotterplättchen) of the eggs of Ray and of fish (loc. cit.). We should mention too, that J. Müller has seen and drawn the granules of the smooth Ray and of the smooth Hound-fish (*Galeus lævis*) (Dotterkörner), Mem. Acad. de Berlin, tome xxvii, page 221, pl. 5, 1842.)

We shall give, in the complete work which will be published with plates in the Archives du Museum d'Histoire Naturelle, a detailed account of the researches of our predecessors.

Of the eggs of Crustacea.—Coloring matter of the Crustacea. The Crabs of our soft waters and Lobsters have supplied the eggs necessary for our researches. Lobsters, carrying from fifteen to twenty thousand eggs under their abdominal appendages, are the most convenient for the kind of researches which we have undertaken.

Their eggs do not contain ichthulin; no sort of granules are found in them. They are essentially formed of an albuminous and saline liquor, holding some fatty bodies suspended in it. The albumen of the eggs of Crustacea seems to us different, in some respects, from the albumen of other eggs. Its coagulation begins about 74° C.; the study of this substance will necessarily find place in the work which we are now preparing on albuminous substances.

We have also studied the Sea Lobster (Palinurus). This Crustacea, as plentiful as the ordinary Lobster (Homardus) on granite shores, and not touching the chalky cliffs, is very uncommon in the North. The Sea Lobster does not seem to pass the Islands of Ushant, and is not found in the British Channel. Further, it is very common on the rocks of Bretagne. It lives at a much greater depth than the common Lobster, for it is necessary, in order to take them, to let down the baited hooks to a depth of seventy fathoms. The eggs of these Crustacea are very small, hardly as large as a seed of the poppy. We have counted about 130,000 of them under their abdominal appendages. We had, in the early part of March, a living Sea Lobster, whose eggs were so developed that the two black eyes of the little fœtus were distinguishable through the shell. We have to regret that we could not save it alive, so as to see the young hatch, to follow the phases of their metamorphosis. Science already possesses some observations made on the embryo of the Lobster, but it has none as yet registered on the development of the eggs of the Sea Lobster, and many other Crustacea.

The study of the eggs of Lobsters enabled us to obtain in a state of purity, that curious matter which takes a red color when the shell of Crustacea is submitted to the action of heat. Till now it has not been conveniently studied, because the solvents always presented it in a state mixed with fatty bodies, and besides, the solvents such as ether or alcohol, give it in the reddish condition, that is already modified; it exists dissolved in the albumen of the eggs of Crustacea; by heating the liquid, the albuminous matter coagulates, carrying with it, in the shape of a lake, the coloring matter, which is then of a very beautiful red. The precipitate is retaken by the alcohol, which dissolves the coloring substance, and leaves the albumen in an insoluble state. The detection of this coloring matter, in the egg of the Crustacea, is undoubtedly an interesting fact, if it be remembered that

art has already taken advantage of this substance. The following method enabled us to obtain the coloring matter as it exists in Crustacea, still presenting its green color.

The green coloring matter of Crustacea is soluble in albumen; besides, when Lobster's eggs are crushed, the albuminous liquid which passes by filtering, is strongly colored green, and holds in solution the coloring matter. The ordinary methods, such as the action of heat, that of neutral solvents, desiccation, &c., which are applied to extract this singular substance, present it already modified. In fact, when alcohol or any other agent is used, the substance which till then, was green, instantly turns red; but by an unexpected and for our work, very fortunate circumstance, the only liquid which does not alter this coloring matter, that is water, enabled us to obtain it in a pure state; in fact, when the green albuminous liquor mentioned above, was weakened with a large quantity of water, the coloring matter is precipitated, and can easily be gathered on a filter. We have thus been enabled to establish the following properties, which we think, are of a nature to interest men of science. This substance is green, resinous and uncrystallizable; it changes and becomes red under very curious circumstances. To effect this transformation, it suffices to submit it to desiccation, even at the ordinary temperature. The salts which have affinity for water can transform the green coloring matter of Crustacea into a red substance: the salts, on the other hand, which do not combine with water, exercise no action on this singular coloring matter. The action of a vacuum rapidly produces the red color. Simple rubbing immediately reddens the green substance. Alcohol, ether, the acids, effect the same change. There are not, to all appearances, in the vegetable or animal organization, coloring substances comparable with this of the Crustacea; modifying with such facility under the action of the simplest agents. We tried to determine whether this substance presented the same characteristics when still fixed in the shell of Crustacea; the result of our experience is, that in this case, the green substance bears itself just as it does when isolated. Thus a Crab's shell, which shows the green discoloration, becomes red the moment it is rubbed with a hard body. It is not the heat developed by the rubbing which effects this singular modification, for it has been effected while the shell was very damp, and besides, it is manifested rapidly on a Crab's shell placed under the receiver of an air-pump and submitted to the action of the vacuum. The change of color by rubbing explains how anatomists who have sought for the cause of the discoloration of Crabs when boiled, have always seen the red matter under the coat, as thin as the epidermis which they removed to observe the shell under the microscope. The simple rubbing of the utricles, touched by the scalpel, sufficed to make the green color red. We are fortunate

thus to have been able to complete the history of one of the most curious coloring matters produced by animal organization.

Of the eggs of Arachnids and Insects.—We submitted to analysis eggs of different species of spiders: they contain albumen, fatty bodies and a large amount of a substance precipitated by water. Ant's eggs gave us the same results. These researches are to be continued in the course of the coming season.

Of the eggs of Mollusks.—The analysis of Snails' eggs, which we shall complete next season, seems to show us that the eggs of Mollusks differ entirely in their composition, from those of other animals. Those which we procured, presented no trace of fat; they were made up exclusively of hyaloid membranes containing a viscous colorless liquid. This liquid contains in solution an organic azotized substance, not albumen, for it did not coagulate with heat. It is precipitated by acetic acid and dissolves in hydrochloric acid without any violet color.

Conclusions.—We have shown, in three successive communications composing our memoir, the facts established by our researches on the eggs of different animals, belonging to all the great classes of Ovipara. Let us by way of recapitulation, endeavor to state in some general propositions, the most important consequences which seem to be the results of this first work. We have shown:—

1st. That there exist fundamental differences between the composition of the eggs of animals, and that under this collective name of *egg*, designating the product of the ovarian apparatus intended to contribute to the perpetuity of the species, very diverse bodies are comprised different as possible from one another.

2nd. That among the vertebrated animals, the eggs of birds, of reptiles, and of fish, present in their composition, differences which the simplest analysis cannot mistake, and besides that the eggs of Sauria and Ophidia bear great analogy to those of birds, while the eggs of Batrachia resemble those of the cartilaginous fishes.

3d. That the eggs of Arachnidæ and insects differ altogether, as to their composition, from the eggs of other animals.

4th. That those of Crustacea, organized for living in water, do not at all resemble those of fish or of other amphibious vertebrata.

5th. That this extends to the eggs of Mollusks.

6th. That these differences correspond not only to classes or orders; that they extend to natural families even, without stopping there, since we have proved that an egg of a cartilaginous fish has not the same composition with that of an osseous fish; but further, that a Carp's egg is very different from a Salmon's egg; that the egg of an Ophidian such as an adder's, does not contain the same principles as those of the Chelonia.

7th. That if the composition of different proximate principles is the same in very nearly allied species, the form and the size of vitellin granules vary in a manner sufficiently appreciable to be able to be recognised and assigned to each species.

8th. That the albuminous substances furnished by eggs of birds, reptiles, fish, crustaceans, present in their chemical properties and in their point of coagulation, differences which permit us to suppose that these bodies are made up of different proximate principles.

9th. That an egg changes its nature,—that its liquids alter considerably at different epochs of its formation, when detaching themselves from the ovary, and resting in the oviduct before being hatched.

10th. After having established in the eggs of different animals, the presence of several new proximate principles, ichin, ichthulin, ichidin, emydin, and comparing these results with those which MM. Dumas and Cahours obtained in the analysis of hens' eggs, we do not hesitate to propose to science to admit the existence in eggs of a new class of organic bodies, comprising some proximate principles which we will hereafter designate under the name of Vitelline substances or Vitelline bodies.

ART. VIII.—*Observations on the Extent of the Gold Region of California and Oregon, with notices of Mineral localities in California, and of some remarkable specimens of Crystalline Gold;* by WM. P. BLAKE, Geologist of the U. S. Pacific R. R. Survey in California.

THE information contained in the following brief notes was partly obtained in connection with the government exploration in California for a practicable route for a railroad to the Pacific, and partly during a visit to the mining region and a residence of several months in San Francisco. The localities and minerals found in the course of the explorations for the government will receive a more extended notice in the report now in preparation.

GOLD.—It is not yet possible to state the boundaries of the great gold-field of California. It has been gradually expanding on the north, west and east with the progress of exploration, until placers have been worked under the snows of the high ridges of the Sierra, and it appears probable that the crest of that great mountain chain is overlaid by the precious dust. On the north and west, new placers of unequalled richness are constantly discovered, and gold is brought from nearly all of the numerous ranges and ridges that trend off from the upper or northern portion of the Sierra Nevada, and traverse nearly the whole breadth

of the State and a portion of Oregon, reaching the coast between Cape Mendocino and the Umpqua river in lat. 43° 45′.* The placers are therefore no longer confined to the State of California but extend into Oregon, not only to the Umpqua river but beyond it, throughout both Oregon and Washington Territories to the parallel of 49°.† Of this northern portion of the gold region there is however but little known, and the latitude of the Umpqua river may be regarded as the northern limit of general mining operations for the present. On the south, the limits of the field have been extended nearly to the Tejon pass at the head of the Tulare valley in lat. 35.° This point is about forty miles south of Kern river where, according to the recent intelligence the placers are rich and are exciting considerable attention. This river rises in Walker's pass (lat. 35° 39′), and flows westward over a broad area of granitic rocks to the Tulare valley, where it empties into the most southern of the Tulare lakes. South of the head-waters of this river the crest of the Sierra Nevada gradually deflects to the west and the breadth of the exposure of granitic rocks decreases, until at the Tejon, the slopes of the Great Basin and the Tulare valley are only thirteen miles distant. The auriferous slates, (talcose slates,) are not found in the section at the Tejon pass, and this may be considered as the southern limit of the Sierra Nevada gold field.

It is more difficult to determine even approximately the eastern and western boundaries of the auriferous area. The elevated portions of the Sierra having been but slightly explored, its eastern limits are not yet defined. Its western margin along the Sacramento and San Joaquin is better known, but is exceedingly irregular. The greatest expansion of the field from east to west is probably in the northwestern part of the State where gold is found from Mt. Shasta to Gold Bluffs on the coast south of the Klamath (lat. 41° 30′)—a distance of 110 miles.

Farther south on the Yuba river the breadth in the direction of the general course of the river is not less than fifty miles. This is believed to be its average width for a long distance southward or to the vicinity of the San Joaquin, beyond which, its limits become more and more contracted to its final termination at the end of the Sierra Nevada. The average breadth of the field for its entire length may be said to be not less than fifty miles.

* It is interesting to observe in this connection that when Prof. J. D. Dana, the geologist of the U. S. Expl. Exp., passed rapidly over the section of country in 1841, he noticed that the rocks gave indications of the presence of gold and in an article published in this Journal in 1849, he mentions the region as follows: "It is quite probable from indications observed by the writer that gold may be found in many parts of the range of country between the Umpqua and Sacramento,"—Am. Jour. Sci., [2] xii, 262.

† This statement is made on the authority (verbal communication) of Dr. John Evans, geologist of Oregon and Washington Territories.

If, for the convenience of making an approximate estimate of the area of that portion of the auriferous belt that is now partly explored and worked, we assume the latitude of the mouth of the Umpqua river (43° 45′) as the northern margin, we find that placers are worked over nearly nine degrees of latitude; or if the distance be measured in the direction of the trend of the field, the region is over 700 miles in length. Multiplying this length by the average breadth (50 miles), we have as the result, an area of 35,000 square miles. If, instead of the parallel of 43° 45′ the parallel of 49° is regarded as the northern limit of the gold region, it extends over fourteen degrees of latitude or over about 1100 miles. According to this estimate a very considerable portion of the field lies to the northward of the state line of California (lat. 42°) and is within Oregon and Washington Territories. There is evidently no natural separation or boundary in the auriferous region of California and Oregon, and it must therefore be regarded as one continuous field.

It is desirable for convenience of description and reference that the whole gold region should be known by a suitable name, and that it should be separated into convenient geographical divisions.* Our knowledge, however, of the region north of the California line (or north of the Umpqua) is yet so limited that it is useless to propose any division beyond the general one of *Oregon Mines* and *Washington Mines*, which will naturally be adopted as explorations extend over those Territories.

In California the gold district extends through, and in some cases includes the following Counties: Klamath, Trinity, Humboldt, Siskyon, Shasta, Bute, Sierra, Yuba, Nevada, Placer, El Dorado, Sacramento, Calaveras, Tuolumne, Mariposa and Tulare. The counties of Tuolumne, Mariposa, and Tulare are each large enough to form several of the size of Placer and El Dorado.

It is already the custom in California to speak of the mining region south of Stockton as the *Southern mines:* this name passed into general use before the placers in the northwestern corner of the State were discovered. In 1854 a map of the *Northern* and *Middle mines* was published in San Francisco, but no boundaries for these divisions were given.

The division of the California district into three great and nearly equal areas with the above appellations, is certainly desirable, and an excellent natural boundary between the northern and middle divisions is found in the upper Sacramento and Pitt rivers; while the Calaveras river is perhaps the best that can be

* It is desirable that a name for the gold field should conform to that which may be given to the great mountain chain of which the Sierra Nevada forms but a part. The writer is engaged in preparing a description of the mountain chains of California, with a view to their classification and nomenclature. The appellation *Columbian Chain* has been suggested as a suitable one for the great line of elevation referred to.

selected as the dividing line between the middle and southern. I therefore suggest that these names be adopted for the areas within the boundaries that I have given.

The Northern mines and those in Oregon are now very productive and important: the gold is considered to be superior in its quality and generally commands a high price among the purchasers. The facilities for access and transportation to them from the Sacramento valley and from the coast, are better than in the Southern mines. A large portion of the supplies is sent to the interior towns from Crescent City and other ports on the coast. Mining operations are conducted at various points on the beach, from famous Gold Bluffs—about 30 miles south of Crescent City—to the Umpqua river. The most important localities are in the vicinity of Port Orford between Gold river (Rogue river) and Coose bay, a distance of 80 miles. Gold is found over the whole distance in the beach-sand from the surface to a depth of six feet or more; it is in very small and thin scales and separates from the black sand with difficulty. Platinum and the associate metals, iridosmine, &c., are found with this gold in large quantity; and as they cannot be separated from the gold by washing, its value in the market is considerably lessened—indeed it is sometimes difficult to make a sale of the mixed metals in San Francisco.

The black sand is found in enormous quantity, it is very deep, and is irregularly stratified by the tides. It is undoubtedly stirred to a considerable depth by the surf during storms, and this is shown to be the case by the fact that the richness of claims that have been worked is renewed during high tides or a storm.

Placers of San Fernando and San Francisquito—Santa Barbara? Co.—This locality of gold has hitherto received but little attention, although it was known to the Californians long before the gold of the Sacramento valley was discovered. These placers are on the southern flank of the mountains that have a nearly east and west trend from Point Conception to San Bernardino and from the southern boundary of the Great Basin and the Tulare valley.

These placers are about fifty miles southeast of the Tejon pass and eighty south of the Kern river placers; they were worked near the ranch of San Francisquito by Mexicans in 1840, and were abandoned when the reports of the great discoveries at the north reached them in 1849.

Talcose slates apparently auriferous and resembling those of North Carolina, occur in the pass of San Francisquito and are traversed by quartz veins. It is reported that veins of auriferous quartz in that vicinity were worked simultaneously with the placers.

An occasional excitement is produced by glowing reports from this locality, and according to recent accounts new placers have been found in the vicinity of Los Angeles. Although we are not yet aware of the extent of these placers, it may be safely asserted that they will not compare favorably in area or richness with those of the Sierra Nevada. They are comparatively local in their extent, but the region is worthy of a careful examination.

Gold of the Great Basin—Tulare Co.—Armagosa Mines.—A vein of auriferous quartz traverses one of the granite ridges of the Great Basin near the Mormon trail to Salt Lake and about 170 miles from Los Angeles. The vein has been prospected and attempts to work it have been made by several companies organized in San Francisco, but it is now abandoned.

The gold is found in wire-like filaments ramifying through quartz and carbonate of lime. I have an interesting specimen in which a string of gold traverses a rhombohedron of carbonate of lime and protrudes from its opposite faces. The form was reduced to a rhombohedron by cleavage.

The occurrence of gold in place, in one of the ridges of the Great Basin, so far removed from the Sierra Nevada gold field, is an important fact, and renders it more than probable that extensive placers will be found throughout its length and breadth.

Colorado River—San Diego Co.—It has been frequently reported that gold exists along the Colorado river not far from Camp Yuma at the mouth of the Gila. I could not obtain satisfactory evidence of the truth of this statement; but if it does occur, it is far from water and vegetation and prospectors are obliged to carry the earth they wish to test many miles before water enough to wash it out can be found.

On the western slope of the mountains between San Diego and the desert, there are good indications of gold at several points west of Santa Isabel and near the travelled road. The region is worthy the attention of prospectors.

Coast Mountains, Santa Cruz Range.—According to J. B. Trask,* gold has been found in the Coast mountains, in the counties of Monterey, Santa Clara and San Luis Obispo.

AURIFEROUS QUARTZ.—Quartz veins are found in great numbers traversing the slates, the granite and greenstone rocks of various portions of the Sierra Nevada gold field; but comparatively few of them have been worked to any extent. Among those that produce the most interesting specimens, the following may be mentioned.

Nevada Co., Lafayette and Helvetia Mine.—Beautiful plates and angular masses of gold are found imbedded in snow-white quartz in this mine. They are frequently intimately associated

* Report on the Geology of the Coast Mountains. Sacramento. Senate Doc., No. 9. Sess. of 1854. p. 58.

with brilliant crystals of white iron pyrites, and in other specimens the gold is entirely isolated from the sulphurets and is surrounded on all sides by the compact opaque quartz. Specimens of this character are sought after by jewellers and are cut and polished for ornamental purposes. An immense quantity of "quartz-gold" is ground and polished in San Francisco into shapes suitable for rings, bracelets, cane-heads and the like. Specimens for this purpose are mostly obtained from the placers, but some of the purest and most brilliant are procured from this mine.

Nevada Co., Grass Valley—Gold Hill Mine.—Extraordinary specimens of gold in large smooth plates have been found in one of the veins of this mine. They traverse a semi-crystalline quartz, and in some specimens the gold is crystalline and is implanted among the ends of the quartz crystals.

Specimens that I obtained, have many interesting mineralogical peculiarities and appear to throw light upon the phenomena of the deposition of the metal.

Nevada Co., Grass Valley—Ophir Hill.—The Empire Company are working a quartz vein at this place, which bears a large amount of white iron pyrites, remarkable for the quantity of gold it contains. If a specimen of the pyrites which does not appear to contain gold, is placed in strong nitric acid so that the sulphuret is partly dissolved, gold becomes visible in many points, showing that it exists in a form favorable for collection.

Tuolumne Co., Marble Springs (Merced River).—An interesting quartz vein at this locality bears plates of gold, iron pyrites, galena and zinc blende. These mineral are abundantly disseminated in a compact gangue of white quartz and form beautiful specimens for cabinets.

Placer Co., Volcanoville.—This place is opposite Forest Hill on the middle Fork of the American river, and is noted as the locality of one or more very rich quartz veins. They occur traversing slates in connection with erupted rocks and contain iron pyrites and free gold, disseminated in irregular masses. Specimens of unusual value and weight have been broken out of parts of one of the veins, and gold can now be found ramifying through the quartz where it has been uncovered and brought to view by the removal of the earth from the top of the vein.

Placer Co., Georgetown—Mamaluke Hill.—A quartz vein of unusual richness occurs at this place traversing the talcose slates and apparently conforms to the bedding. It is narrow and much decomposed, the quartz being cellular and friable, and is highly charged with irregular filaments and ragged masses of gold. The decomposition and discoloration of the vein and the adjoining slates rendered it so obscure that it remained unnoticed while the claim was worked as a placer mine. I had the satisfaction of pointing out its true character and washing out the first prospect,

which amounted to several dollars. Within a few weeks after this discovery, the owners of the claim were taking out 274 ounces a week, sometimes finding four ounces in a pan. The last accounts that I have received state, that sixty-five thousand dollars worth of gold had been obtained—all from an ordinary mining shaft fifty feet deep, and without the aid of machinery.

Crystalline gold.—Good crystallizations of gold are comparatively rare in California. A hundred pounds of coarse placer gold may be carefully examined without finding a single well formed crystal. This may partly result from the custom among the miners of reserving any peculiar or remarkable specimens of small size for breast-pins or for preservation as curiosities.

Placer Co., Forest Hill.—Interesting octahedral crystals have been found in the claims of the Messrs. Deidesheimer at this place. These crystals occur with placer gold 2500 feet above the level of the river; but they are not much worn by transportation. Quartz crystals are found mingled with the auriferous earth with only their sharp edges and angles removed by attrition, which shows that the drift is comparatively local and indicates the presence of a parent vein in the vicinity. Most of the crystals have the peculiar triangular depressions in the faces, generally found in gold crystals, and some of them are very much distorted; others are flattened parallel to a face so as to become thin triangular plates. These specimens are seldom more than three-eighths of an inch across the base.

An imperfect octahedral crystal of extraordinary size was taken from the claim last year. The planes are only partially developed for a short distance above and below the basal ridges, and the peculiarity of a series of similar parallel planes, lying like plates one within the other, is presented.

The crystal is elongated in the direction of a line parallel with two basal edges and thus becomes a rectangular octahedron.

The length of the longer base is one inch, and the shorter seven-eighths of an inch. I believe this to be the largest crystal ever reported; it may be called a *skeleton* crystal on a grand scale.

Arborescent and Dendritic Gold—Placer Co.—Some of the most remarkable and beautiful specimens of gold ever seen, have been found at Irish creek three miles from Coloma. They simulate the veined and reticulated appearance of leaves and more closely resemble the foliage of the Arbor Vitæ or the fronds of the most delicate ferns than of any other forms of vegetation. The filamentous and arborescent masses are frequently united to plates (as broad as the hand) which are covered with lines of crystallization and are brilliant with numberless faces of partly formed crystals. They are also combined with good crystals which are generally octahedral and have perfect faces.

I have a very beautiful specimen of this character in the form of a leaf: one side is beautifully arborescent, and the other is studded with perfect octahedrons of various sizes and about twenty-five in number, including the smallest. They are geometrically arranged, all their similar edges being parallel. This is believed to be the most remarkable and beautiful specimen known. Its weight is 17 pwt. 10 grains. Length, two and one quarter inches, width, one and a half inches.

One of the foliated specimens in my collection, bears a crystal having the form of a pentagonal dodecahedron with cavernous faces.

One of the largest specimens of this arborescent and foliated gold that has been procured, was about twelve inches long and about four broad. A part of the specimen was a plate three or four inches long, covered with triangular marks; the remainder was arborescent, and the whole appeared to have grown from one end.

Another specimen slightly different in its character and probably from another locality in the vicinity, was ten inches long, three broad and about half an inch thick. It weighed 31 ounces, and was free from quartz; forming a most beautiful mass of a rich yellow color and a delicately marked surface, consisting of a net-work of fibres. It appeared like a bundle of broken fern leaves closely matted together.

These specimens are evidently from a quartz vein, but although I have visited the locality, I have not been able to see the place from which they were taken, or to obtain any reliable information concerning their mode of occurrence and the associate minerals. Some of the foliated specimens were incrusted with a thick scale of sesquioxyd of iron.

The locality is about three miles from Sutter's mill—the point where the gold was first discovered.

Platinum.—The occurrence of this metal and its associates with the gold of Port Orford, has been previously noticed in this Journal.* It appears to constitute a large percentage of some of the samples of gold brought from that region and this renders the locality peculiarly interesting. Several ounces of the mixed metals were obtained, and are now being carefully examined. The platinum is in very small thin scales and is easily lifted by a magnet. The difference in the specific gravity between this metal and gold is beautifully shown by the newly invented separating machine,† which distributes black-sand, gold and platinum in separate zones at the upper edge of the oscillating copper table.

I have found platinum in small quantities in the gold of the Middle mines, and in the fine scale gold from the forks of the American river, but it appears to exist in small quantity. It is

* Second Series, vol. xviii, p. 156.
† Invented by Horatio Bradford, Esqr., of New York.

an interesting fact that the metal is more common in the Northern mines, and that it is most abundant on the coast.

Mercury.—*Santa Clara Co., New Almaden.*—The ore at this mine is a massive sulphuret (cinnabar) and its character and association have already been described in this Journal, vol. vi, p. 270, and xvii, p. 438.

The rocks at the locality appear to be metamorphosed sedimentary strata: They crop out at several places below the mine on the side of the hill, and consist of regular strata of argillaceous shales and layers of flint and jaspery rock, which resemble those of San Francisco near the Mission and Fort Point. Serpentine rock is found in, and near the mine and trappean rocks are also found in the vicinity.

The similarity of these strata and the serpentine to those of San Francisco leads me to consider them as of the same age at each place, and it is probable that the flint and jaspery rock is a metamorphosed portion of the blue sandstone formation of San Francisco.

Guadalupe Mine.—This is another locality of cinnabar about three miles from the New Almaden mine, but it is not now worked.

Monterey Co.—Other localities are reported in this county and at one point a vein has been opened by parties residing in Monterey. I have no definite information of its extent, but specimens of the ore of fair quality were exhibited in Monterey.

Copper.—Small amounts of copper pyrites and green coats of the carbonate resulting from its decomposition, are found in many of the veins of auriferous quartz that have been opened in different parts of the State. I have not, however, yet heard of any locality that promised to reward explorations conducted for the copper alone. According to J. B. Trask, sulphuret of copper and the blue and green carbonates occur with quartz in Monterey Co. at Alisal; also in Santa Barbara Co. and San Luis Obispo.

A vein of copper pyrites occurs on the slope of the Great Basin about seven miles east of Johnson's river. It is associated with quartz and by its decomposition has produced abundant green crusts of malachite. Oxyd of iron in fine powder is found in the cells and cavities of the quartzose gangue, and appears to have resulted from the decomposition of the pyrites. It is very probable that this vein is auriferous, but the specimens have not yet been examined. The vein traverses a micaceous granite and appears (from the outcrop) to have a width of over twenty feet, including the quartzose gangue. This is a valuable vein, and doubtless will show some splendid ore when thoroughly opened. Its discovery is one of the results of the geological reconnoissance in connection with the R. R. Survey.

Copper pyrites is also found in a vein about seven miles below the summit level of the New Pass, which leads from the Great Basin to the valley of San Francisquito. This vein outcrops on the southern slope of a granite hill on the north side of the Pass, and is about 90 feet above the bed of the creek. The ore resembles in its luster and color the variegated copper pyrites, but is much softer. It is found in strings and narrow veins distributed through a hard quartzose gangue about fifteen feet thick: the thickest seam of ore, however, does not exceed two inches; but when several such were closely combined a thickness of eight inches of good ore was seen.

This vein has been prospected and a small quantity of ore broken out. It is about sixty miles distant from Los Angeles by the trail.

Native Copper and Red Oxyd of Copper.—When visiting Camp Yuma at the junction of the Colorado and the Gila rivers in Dec., 1853, several large masses of superior copper ore were shown to me by the officers of the fort. This ore was brought from the adjoining State of Sonora, Mexico, and the vein is reported to be near Altar. It is within the limits of the strip of territory recently acquired by purchase and is therefore now in the United States. Specimens are frequently brought in by emigrants who cross the Colorado at the ferry below the fort. The ore is principally the red oxyd of copper associated with the pure metal and green crusts of carbonate. The specimens that I saw ought to yield about ninety p. c. of pure copper. This is probably the ore that has recently excited so much attention in California, and has been reported to be highly charged with gold.

Calaveras Co.—Native Copper and Silver.—A specimen of pure copper combined with silver is reported to have been found in a placer mine near Mokelumne Hill. The specimen was exhibited in several places and sent to San Francisco, but I have not been able to obtain any reliable information concerning it, or the circumstances under which it was found.

Iron ores.—*Mariposa Co., Burn's Creek.—Limonite.*—An outcrop of hydrous sesquioxyd of iron or limonite occurs near the banks of this creek, on the right of the road going south. It is associated with a quartz vein and forms a bed about twenty-five feet thick, lying conformably with talcose and chloritic slates. The ore outcrops in great solid blocks from two to four feet in diameter: it is compact, of a dark brown color, and breaks with a smooth conchoidal fracture. The position and peculiarities of this ore indicate that it has resulted from the decomposition of pyrites, and that it forms the "gossan" of a vein of sulphuret below the surface. The mass does not, however, present that cavernous and friable condition in which gossan is generally found.

Magnetic Iron Ore.—A massive and fine-grained variety of this ore is found associated with one of the auriferous quartz veins of the county. Specimens that I have seen were colored green by their layers of carbonate of copper. It has polarity and lifts small fragments.

Tulare Co.—Magnetic Iron Ore.—This ore occurs in a bed or vein about three feet thick in a low ridge of white crystalline limestone at the summit of the pass known as the Cañada de las Uvas. The ore is compact but not crystalline, when broken it shows a brilliant fracture and a granular surface, and does not break with flat faces, like the massive magnetic ores of New York and New Jersey.

New Pass—Magnetic Iron Ore.—Specimens of very pure and highly crystalline magnetite were picked up in the valley of this Pass: it is associated with hornblende, cinnamon-colored garnets and chlorite.

Placer Co., Volcanoville.—Large boulders of compact magnetic ore occur in the bed of the creek that flows by the side of the great vein of auriferous quartz at that place. These boulders are so large and abundant that it is probable a vein of ore will be found in sites in that vicinity.

San Francisco Co.—The fissures and crevices in the serpentine rocks of San Francisco are occasionally drusy, with small but brilliant octahedral crystals of magnetite. They did not give reactions for chromium when examined before the blowpipe.

Sulphuret of Iron.—Good crystals of pyrites are obtained in the talcose slates in various parts of the mining region. At Georgetown, Placer Co., it is abundant in minute cubic crystals. They are obtained free from rock or gangue as one of the products of gold washing, and as they are very brilliant and of uniform size they are worthy of a place in good collections.

Chromic iron.—*Monterey Co.*—Massive chrome ore of excellent quality was shown me in San Francisco and reported to be from a short distance south of the Mission of San Juan. It is an interesting fact that it is almost identical in its appearance with the ore from "Wood's pit" in Maryland and like it, is partly covered with green coats and crusts of emerald nickel. The extensive distribution of this mineral in California has been noticed at length by Mr. P. T. Tyson in his report.*

Antimony.—*Tulare Co.*—A large vein of the sulphuret of antimony, (antimony glance,) exists in the high granitic range that borders the Tulare valley on the south. It is about eighty miles from Los Angeles and is most readily visited from the Tejon. By observations with the barometer, I found the outcrop of this vein to be at an altitude of about 6000 feet above the sea. It is on the side of a precipitous ridge of granite and not favorably situated for examination. Its thickness was estimated to be ten feet

* Ex. Doc., No. 47. 31st Cong., 1st Session. (Senate.)

or more. A steep chasm or channel extends from the top of the ridge to its base, and is partially filled with rocks and the debris of the vein. Solid blocks of the ore were found with this accumulation, having been broken out from the vein above; one of them was twenty-seven inches long and sixteen to eighteen wide.

The ore is associated with quartz, and where it has decomposed, an abundance of antimony ochre is found, together with crystals of selenite. Specimens of quartz traversed by long prismatic crystals of the ore were obtained.*

Salt.—Salt is found in small quantity as an incrustation or efflorescence on the soil along streams or on the margins of ponds in nearly all parts of California. It appears to be most abundant in connection with the tertiary strata and in the streams that flow from them. It is doubtless the fact that a great part of the incrustations called soda, consist principally of common salt.

Tulare Co.—Cañada de las Uvas.—There is a small shallow lake near the central part of this Pass fed by springs and streams from the adjoining valleys and ridges which are partly of tertiary strata. During the summer season the water of this lake evaporates, and its bed becomes covered with a white crust of salt which glitters in the sunlight like a field of snow.

Taheechaypah Pass.—A lake of a similar character to the one just described is found in one of the elevated valleys of the Sierra Nevada near this Pass. At another locality in that vicinity and near the margin of the Great Basin, salt occurs in a thick bed, from which over one hundred mule-loads have been taken, and carried to the Tejon Indian reservation for the use of the Indians.

This salt is perfctly white and amorphous, being reduced to a fine powder by simple pressure. It is sufficiently pure for table use.

Dry salt lakes are also found near the termination of the Mojave river in the Great Basin, and at many other places throughout Southern California.

Los Angeles Co.—Salt is now manufactured in large quantity from sea-water by solar evaporation on the coast near Los Angeles.

Lower California.—A dry salt-lake has been discovered about 250 miles south of San Diego and near Marguerita bay. It forms a thick bed and is very pure, being well crystallized in large hopper-shaped crystals. It is reported that the locality has been purchased by capitalists and that the salt is being shipped from there in large quantity.

Gypsum.—Transparent plates of selenite are common in the soft unconsolidated tertiary strata in various parts of the state. At some localities it forms seams or beds several inches thick lying conformably with the stratification. In Tulare Co., at Ocoya

* A more detailed notice of this locality will be found in the author's Preliminary Geological Report accompanying the Report of a Reconnoissance and Survey in California, by Lieut. R. S. Williamson. House Doc., 129. 1855.

creek these transparent plates are found in the Miocene strata; some of them are combined with the fibrous variety, and form beautiful cabinet specimens. Good crystals are also found in this county, at the antimony localities. Thin transparent plates are numerous in the Miocene strata bordering the Colorado Desert, and on the borders of Carrizo creek they are found lying loose upon the surface where the strata have been worn away by the rains. Seams of gypsum are numerous in the tertiary strata of Benicia.

Wherever I saw the gypsum in the tertiary strata, it appeared to have been formed by the infiltration of sulphates and their decomposition by the lime of the beds.

Bitumen.—The occurrence of bitumen springs in the Coast Mountains has been noticed in the writer's preliminary Report.*

Near the Pueblo de los Angeles, there is a large pond or lake of the bitumen about one quarter of a mile in diameter. Its central portion is soft and semifluid but the outer parts are hardened by exposure. The material is much used for covering roofs, and at Monterey I saw a good basement formed from its mixture with sand and gravel.

Sulphur.—Very interesting specimens of sulphur can be obtained at the Geysers or hot springs in Napa Valley. It occurs in crusts or lining fissures in the soil and tufaceous deposits around the springs, and is in small crystals, forming drusy surfaces.

Beryl?—*Tuolumne Co.*—Small and well formed hexagonal crystals having the hardness and color of beryl have been obtained from the Tuolumne river three or four miles from Jamestown. The specimens that I saw were apple-green, and one of the smallest was emerald-green and transparent. The largest crystal was nearly $\frac{1}{4}$ of an inch in diameter and terminated at both ends with the planes R, and $-\frac{1}{2}$, as in tourmaline. I was unable to retain the specimens for further examination.

Tourmaline.—*San Diego Co.*—Black tourmalines of unusual size (from six to eight inches in diameter) occur abundantly in the huge feldspathic veins that traverse the granite ridges bordering the elevated valley of San Felipe, in the mountains between San Diego and the Colorado desert. These crystals are not perfect.

Feldspar.—Orthoclase.—*San Diego Co.*—Good crystallizations of this mineral can be found in the granite veins near the road between Santa Isabel and San Pasquale. They are associated with tourmalines and garnets.

Andalusite, *Mariposa Co.*—This interesting mineral was found in great abundance in a conglomerate that caps the hills along the Churchillas rivers (San Joaquin valley) at the crossing of the road leading to Fort Miller.

* See also this Journal, [2] xix, p. 433.

Very fine crystals of unusual size occur in the gravel along the bank of the stream. I picked up several that were two inches long and three quarters of an inch in diameter. They have a delicate pink or rose color and some of them are translucent. The peculiar tesselated appearance displayed in a cross section of crystals of this species, is exhibited by these specimens in a beautiful manner.

Calcite.—Crystallizations of this mineral are found at the Quicksilver mine, (New Almaden), at the Pass of Jacum, San Diego Co, and on the surface of the Colorado desert north of Carrizo creek, where some transparent crystals were picked up. It also occurs in beautiful stalactites and delicate crystals in the great cave in Calaveras Co.

Art. IX.—*Analysis of Idocrase from Ducktown, Polk Co., Tenn.;* by J. W. Mallet, Ph.D.

The specimen of this Idocrase examined occurred among some other minerals from the Ducktown copper mine, in thin bladed crystals imbedded in a mixture of copper and iron pyrites. The crystals were from half an inch to two inches in length, about half an inch in width, and not more than an eighth of an inch thick. They were longitudinally striated, and presented scarcely any planes, lateral or terminal, suitable for measurement by the reflecting goniometer. The existing planes however seemed to be 0, 1, ∞, 1-∞, and ∞-∞, the last being most developed, and the others very small. The mineral was nearly colorless, exhibiting but a slight tint of brownish or yellowish green, and was subtransparent. H. =6·5; sp. grav. =3·359, taken with fragments.

Analysis gave the following results, a little copper present being calculated as copper pyrites (derived from the gangue), and the necessary amount of iron subtracted from the total quantity of oxyd of iron weighed:

		Atoms.	
Silica,	38·32	·846	
Alumina,	25 68	·500	
Protoxyd of iron,	8·13	·226	1·151
Lime,	25·39	·907	
Magnesia,	·36	·018	
Copper pyrites,	1·91		
	99·79		

We have here a slight excess of alumina over the amount required by the formula $3RO, SiO_3 + Al_2O_3, SiO_3$, and a slight defect of protoxyds. The fragments of mineral were selected with great care, and contained no visible particles of gangue—hence the copper found in the analysis may perhaps, as in Cyprine, belong to the Idocrase itself, in which case the protoxyds would be increased by tbe copper and iron calculated above as copper pyrites.

Art. X.—*Observations on Binocular Vision*; by Prof. William B. Rogers.

The remarkable discoveries of Prof. Wheatstone in relation to binocular vision, and the observations of Sir David Brewster, Prof. Dove and others in the same field, must be regarded as among the most important contributions ever made to the science of physiological optics. Nor has the knowledge of these beautiful and curious results been confined to the circle of scientific inquirers. The diffusive intelligence of the age has converted the stereoscope into a popular source of instructive recreation. But much as has been done, especially by Wheatstone and Brewster, towards forming a true theory of binocular vision, it must be apparent to all who are familiar with their labors that these observers differ in some important particulars, as well in their account of the phenomena as in their mode of interpreting them.

In the following pages I propose to record the substance of a series of observations, begun some years ago with the view of repeating and varying the experiments, and if possible testing the conclusions previously made known in regard to this interesting subject. It will be seen that the modes of observation on which I have fallen are in some respects new, and have the advantage of great facility, and that the results and the inferences which I have drawn from them, lead to views of the process of binocular vision differing in some respects from those of either of the eminent philosophers to whom I have referred. To form a theory of this process in all respects satisfactory may require further observations and greater subtlety of metaphysical interpretation, but I trust that the facts and conclusions which I have to present will throw light on some obscure points of the subject and will perhaps disclose features of it which have not hitherto been noticed.

PART FIRST.

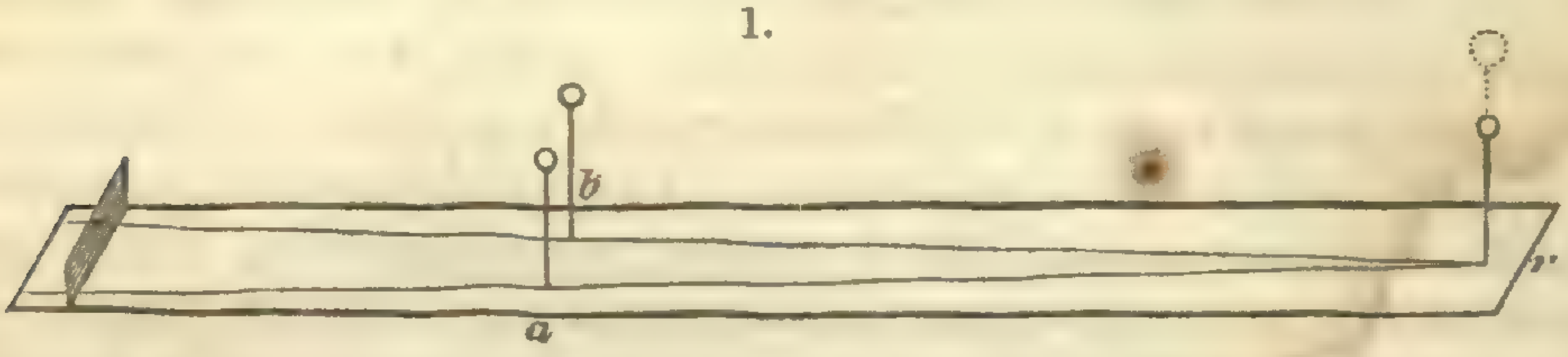
1. *On the place in which we perceive objects that are binocularly combined.*

The learned Dr. Smith of Cambridge, in his "Complete System of Optics," p. 388, describes an experiment in which by binocular vision he united into one long perspective line the legs of a pair of compasses partially opened and held in a particular position directly before him, and he ascribes the apparent union to the fact that the pictures of the corresponding parts of the two legs fall "upon the same points of the retinas as that of the object to which the eyes are directed." It is therefore clear that this writer apprehended the primary Law of binocular combination very much as it is at present understood. Although mistaken in his account of part of the experiments and in other ob-

servations in binocular vision, he is I think entitled to the credit of having first perceived a chief condition of these phenomena.

But the law of the apparent place of objects binocularly combined is so fundamentally important as to call for other and better modes of proof. One such, remarkable for its completeness, has been described by Sir David Brewster (Phil. Mag., 3d Series, Vol. 24), and I may be permitted to add the following simple experiments for the same purpose which I have found very satisfactory and easy of execution.

1.



On a light strip of wood about 3 feet long and 4 inches wide, two lines are to be drawn diverging from one end at such an angle that their terminations at the other end shall be a little nearer one another than the pupils of the two eyes (fig. 1). Three steel pins each about $2\frac{1}{2}$ inches high and surmounted by round heads $\frac{1}{8}$th inch in diameter may then be fixed perpendicularly in the wood, one of them at the angular point, and the two others at points of the lines not nearer than the limit of distinct vision. The instrument is to be brought close to the face, in such position as to enable us at the same time to cover r by a in the right eye, and by b in the left. If now we direct our attention to the remote pin r we see what appears to be another pin thicker and taller than r, coinciding with it or very near it. This is the binocular resultant of a and b as seen by the two eyes converged to r.

To obtain this effect in the most satisfactory manner the unpractised observer will find it useful to conceal the surface of the board by interposing a low screen between a b and the near end of the instrument, or he may effect the object by depressing the remote end until the lower part of a and b ceases to be visible. If, while directing the eyes steadily towards r so as to perceive the resultant image of a and b, we gently vibrate the instrument in its plane around a point midway in a b, we see the remote pin passing across the position of the compound image, sometimes behind and sometimes in front of it, but always very near. This curious result never fails to show itself, if we are careful to direct the eyes steadily upon the image while moving the instrument, and it gives us a clear impression of the place of the resultant as coincident with the point to which the optic axes are converged. It is important to observe that while viewing the compound image in this experiment, we see, on the right of it, the image of

the right pin as visible to the left eye; and on the left of it, that of the left pin as seen by the right eye; and, provided the eyes be steadily fixed on the resultant, these lateral images appear to lie at the same distance and to have the same magnitude as the resultant.

The following variation of this experiment is easily repeated, and illustrates the same law of binocular combination in a very simple and striking manner. Omitting the remote pin and fixing the two others about 18 inches from the near end of the board, and on opposite sides, at an interval not much less than the distance between the centres of the eyes, let the observer place himself 6 or 8 feet from a window commanding the view of a bright wall or a roof covered with snow. Then holding the instrument with its farther end a little depressed, or interposing the screen, let him vary the convergence of the optic axes until he has formed the resultant image, and is able to view it steadily. He will observe that this image seems to occupy a position outside of the window, and he will have the feeling that *he is looking at it through the glass.* Retreating a few steps, he will see it as if situated in the plane of the window, and as he recedes still farther in the same direction, it will appear within the apartment between him and the glass. Throughout these changes he cannot fail to notice that it maintains a constant distance from the eyes, and has for its position the point of meeting of the optic axes.

This effect is even more striking when the bright surface in front is that of a globular astral lamp. In this case by commencing the observation at a distance of 12 or 15 feet from the lamp we see the binocular image on the near side of this object, but as we slowly approach we observe the image apparently *penetrating the luminous globe*, and at a less distance still we see it beyond the lamp as if we were looking through the glass at a real object in that position.

In the preceding cases the resultant is formed by converging the optic axes to a point more remote than the two objects to be united, but it is easy, by a different arrangement of the pins, to prove that the same law holds for the position of the binocular

2.

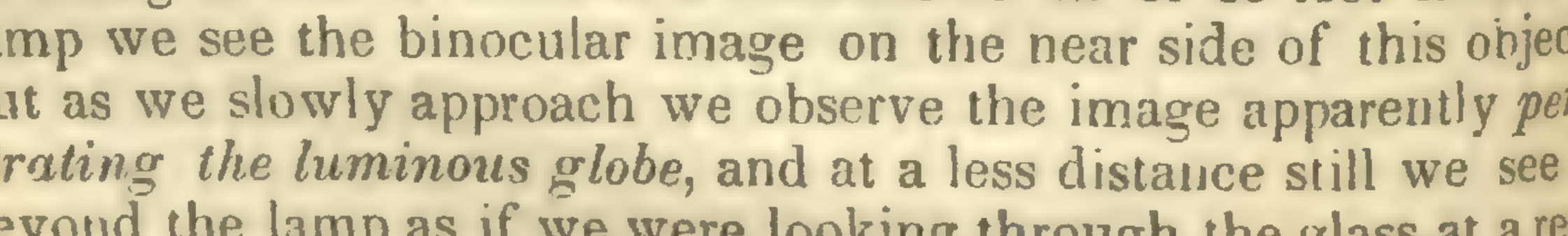


image when the point of convergence is nearer than the objects. For this purpose *a* and *b* are to be fixed at the further end of the board, and *r* at some intermediate point in the central line (fig. 2). Then directing the optic axes steadily towards *r* we see the

compound image placed between the two lateral ones at or very near *r*. By giving the board a slight vibratory motion around the end *a b* we cause the pin *r* to pass alternately to the right and left at a small distance behind or before the image, while the latter is seen to maintain its position at the point of intersection of the optic axes. By making *r* the centre of the vibration we produce a like movement of *r* in regard to the image.

In either of the modes of adjustment which have been described this instrument may be used as a stereoscope. It is true that those observers, who by any practise have acquired the power of converging their optic axes steadily upon near and remote points at will, do not require the aid of a fixed point towards which to direct them, and can readily without any instrument combine the corresponding pictures and obtain the full stereoscopic effect. But most persons find great difficulty in maintaining the necessary adjustment, and need the assistance of some guide-object as the pin *r* in a proper position.

By placing stereoscopic drawings of suitable dimensions in an upright position against the pins *a b* (fig. 1), and directing the eyes to or beyond the remote pin, which in this case should be taller than the others, we see the resultant perspective image at the place to which the axes are converged; or if we rest the drawings against *a b* (fig. 2), and converge the eyes to *r* or to some point readily found, before or behind it, we see the resultant image perfectly formed in that position. It is hardly necessary to say that, in the former case, the points of the twin figures which are to be united, should be at a less distance apart than that which separates the centres of the eyes. When the distance is exactly equal to *a b* (fig. 1), the image will be formed at or near *r*. In the second case this restriction as to the space between the drawings does not apply. When the interval of the corresponding points is equal to *a b* (fig. 2), the resultant picture will be formed at *r*; when the interval is greater, *r* must be moved nearer the eyes; when less it must be moved toward *a b*.

2. *Of the apparent distance and magnitude of the lateral or component figures.*

In all experiments in which the component figures are seen along with the resultant image, an interesting fact is observed in regard to their size and distance from the eye. When first formed the resultant appears to occupy a position in advance of or behind the plane of the compound figures, according as the axes are converged to a point nearer than that plane or beyond it; and as long as the attention is divided between the resultant and the component figures, this relative position is maintained. At the same time, the resultant appears smaller in the former case and larger in the latter than the components. But if we fix the eyes

steadily on the resultant, we see the two other images take their places beside it in the same plane and of the same magnitude as its principal section if it represent a solid.

In making this experiment, it is best to use identical figures, such as two equal bodies about an inch in diameter whose centres are 1½ inches apart. If, while the three figures are thus visible in the same plane, the eyes be suffered to glance sideways for a moment, the component figures retreat or advance from the plane of the resultant, and a renewed effort is necessary to bring them back to it. At first some difficulty is experienced in retaining them in this position, as the observer without knowing it is prompted to glance alternately at the resultant and the components and is thereby led to refer the latter to the distance at which they are actually placed. After some practice, however, it becomes easy, at will, to shift the components into or from the plane of the resultant, and at the same time to observe the change of their apparent magnitude.

A similar phenomena presents itself in the case of the two images of a single object seen on directing our attention to a point beyond it or between it and the eyes. When for example we look steadily at the middle of the bright globe of an astral lamp while holding a wire or slender pencil between it and the eyes, the two images of the pencil appear to touch the globe. If however we fix our gaze steadily on the pencil we see two images of the globe, one on each side of it, and at the same distance from us as the pencil. In the former case the pencil appears larger and in the latter the globe smaller than when viewed in their real position.

It would seem therefore to be a law of vision, that, while the eyes are intently regarding a particular object, all others seen at the same time are referred to the same distance, or, in other words, that *the sense of distance impressed upon our minds by the object to which our eyes are specially directed always connects itself with the other objects seen at the same time.*

In this experiment it is important to exclude all extraneous conditions which are likely to suggest the true places of the objects. Hence the pencil or rod instead of being held in the hand should be supported in some other way, and a screen of black pasteboard or the dark cover of a book should be interposed, in such position as to hide from view the stem of the lamp and the lower end of the pencil, or whatever may suggest their mode of connection with objects near them. For want of this precaution, the learned Dr. Smith and after him most writers on optics have failed to recognise the law above stated ; for he says (p. 48 Complete System of Optics), "it is manifest that the two apparent places of the object are neither of them the same as its real place, and that they lie *between it and the mark we look at* but not very far from the real place."

As the lateral images tend to withdraw the eyes from the compound one situated between them, and thus to distract the observer's attention, it is necessary, for the most satisfactory results, to exclude these from the view. In the simple form of stereoscope about to be described, this is effected in such a manner that by suitable adjustments we may obtain the resultant image either at a greater or less distance from the eyes than that of the drawings from which it is formed. It will be seen that in general plan this apparatus bears some resemblance to the phantascope of Prof. Locke. The latter however is only adapted to the formation of a compound image between the drawings and the eyes, and it does not exclude the lateral pictures.

3. *Description of the sliding stage Stereoscope.*

This instrument is represented in fig. 3 where AB is a thin strip of light hard wood about 3 ft. long and from 2½ to 3 inches broad to which are attached two square pieces C and D of the same material each about 6 inches square, fitting tightly upon the strip by slits at B and *n* so as to be adjustible at various distances from A. In C is cut a rectangular opening *m* about 1¼ inches wide and 2 inches long. A narrow slip is fixed on the stage D at *o* and another on C at *p* for the purpose of supporting the diagrams when the instrument is held in a slightly inclined position with the end B a little downwards.

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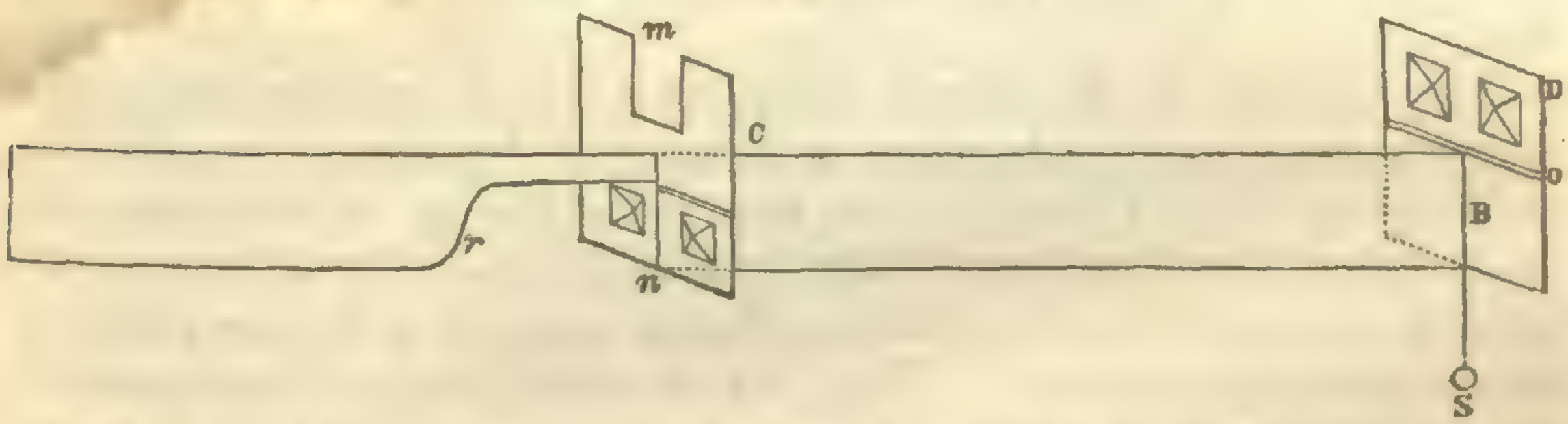

When the diagrams are to be combined at a point farther from the eyes than their actual distance they must be placed at *n* on the stage C and the instrument turned around so as to bring *n* above *m*. Should the distance between their corresponding points be much less than the interval separating the two eyes, the union will be readily effected by bringing A close to the face, midway between the eyes, and looking past the diagrams towards the moveable pin S or to some point between it and *n*, which in the rapid adjustments of the axes is almost instantly found. In this use of the instrument the part A*r* of the central strip conceals both the lateral images, the right eye being permitted to see only the right hand figure and the left eye that on the left hand, and these by a due convergence of the axes are united in a single stereoscopic resultant. When the component diagrams are farther apart or when the stage itself is moved to a greater distance from A, the point of convergence proper to their union is

more remote than S. But in this case the union is easily effected by directing the eyes past *n* to a point in the wall or other distant object in the room, and approaching or receding until a suitable convergence is attained. After a little practice even this becomes unnecessary, as we can then at will direct the eyes as if gazing upon objects at various distances behind S.

In this mode of combination, the picture of the object proper to the right eye is viewed by that eye and the picture proper to the left eye by the left, just as with the common stereoscopes. Hence the drawings intended for the latter instruments may be used in this, provided they are so close to one another as to bring the points which are to be united, nearer together than the centres of the eyes. The ordinary twin-figures on black paper can be adapted to very easy combination by dividing the paper transversely so as to separate the figures and then placing them on the stage at *n* in closer juxtaposition. For the greatest facility of observation they should be brought near enough to admit of being combined by converging the axes to S, or to some point between this and the stage C. This stage is represented in the diagram at the greatest distance from A at which a double drawing on the same paper can be used, but it can be moved up nearly to *r* without disturbing the paper, as in the interval the strip is cut away to a narrow stem. When separate drawings are employed they may be moved with the stage to any desired position between B and A.

The absence of the screen between *r* and *n* enables each eye to see a little into the space commanded by the other, and by moving the stage towards *r* we can so adjust it as to make the central line at *n* the inner limit for both eyes. In this way we have it in our power *to combine figures having a line or point of intersection in common.* This, which is a peculiar adaptability of the present instrument, leads as we shall hereafter see, to some curious stereoscopic effects.

When we propose to form the resultant figure somewhere between the drawings and our eyes, it is necessary to place the twin picture or the separate drawings on the stage D, and to adjust the stage C to such a position that looking through the opening *m* with the right eye we see only the left hand drawing, and looking with the left eye we see only the right hand drawing. If both eyes be then directed toward the opening, we observe in or near it, hanging in the air, the beautifully clear binocular resultant.

The resultant figures thus formed has a relief just the reverse of what it would be with the same drawings placed on the upper stage of the instrument, for in the present instance the right eye views the drawing proper to the left eye, and the left that proper to the right. Hence by simply shifting the paper from one stage

to the other we have the opportunity of alternating these stereoscopic effects.

In these experiments the apparent magnitude of the resultant exceeds that of the drawings when the upper stage of the instrument is used, and falls short of it when the drawings are placed on the lower stage, the change in both cases being determined by the relative distance of the resultant and the drawings from the eyes.

4. *Of the unusual adaptation of convergence and refractive power by which we see the resultant image.*

A feature of great interest in the preceding experiments is the clearness and precision of the resultant image. This is equally true whether the image be formed between the eyes and the diagram or at a greater distance than the latter. In the one case the lines are more slender and in the other case broader than in the drawings, but this is only a part of the general diminishing or enlarging influence due to the relative distance of the resultant and the drawings from the eyes, and does not affect the distinctness of the combined image.

In order that each point of the resulting figure may be thus clearly seen, the eyes must be in a condition suited to the accurate convergence of the rays falling upon them from the corresponding points of the drawings; that is, *they must for the time be adjusted in refractive power to the distance of the diagrams and not to the distance at which the binocular resultant is seen.* At the same time the convergence of the optic axes to a point nearer or more remote than the pictures has the effect of determining our perception of the combined image at that point.

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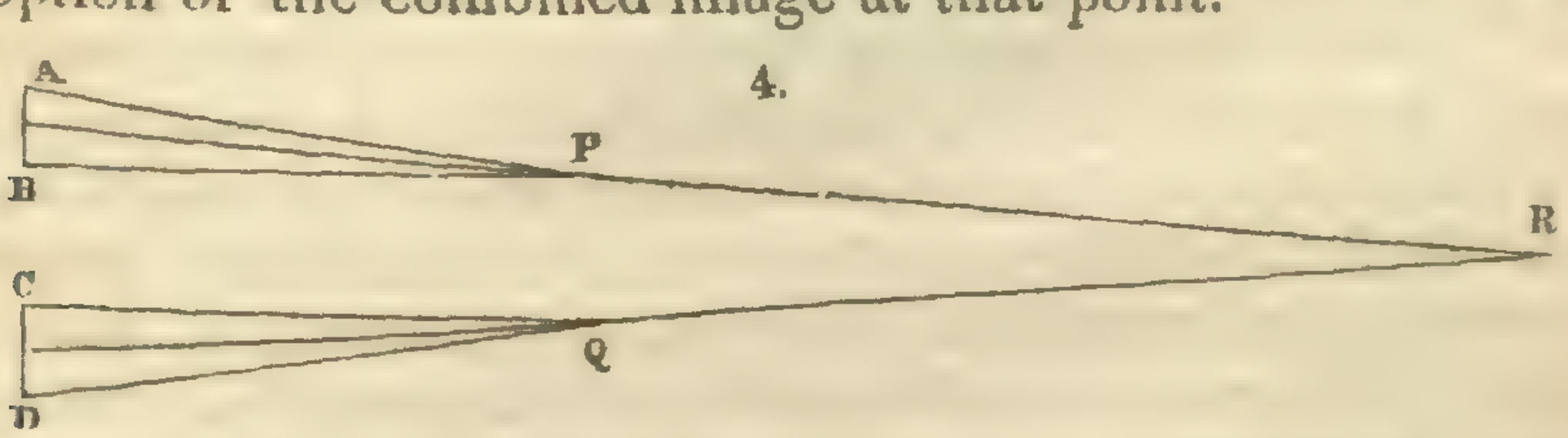

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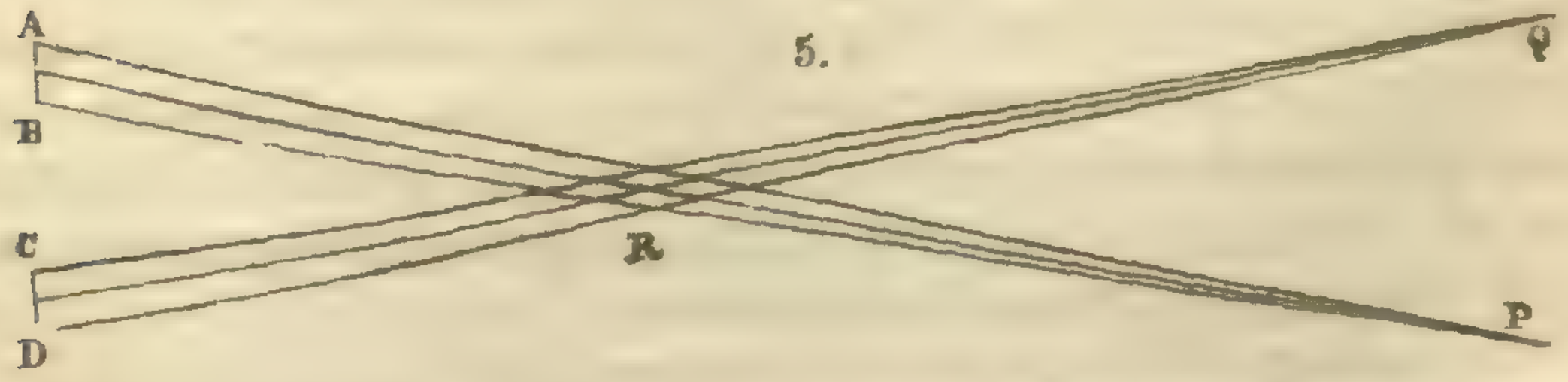


To illustrate this more clearly let P and Q (figs. 4 and 5) represent any two points of the drawings which are seen united at R, and let PAB, QCD be the conical pencils proceeding from them respectively to the left and right eyes. As the resulting image is clear and without blur we are to infer that these conical pencils

are converged to an accurate focus on the sentient surface of each eye at the inner extremity of the optical axis. But that this may be so, the eyes must be adapted in refractive power to the divergency of the pencils or in other words adjusted to the distance of P and Q the actual sources of the rays, although from the convergence of the axes to R we are led to refer the combined image of these points to that position in space.

In these conditions therefore it appears that the act of vision is attended by a peculiar and unusual relation of the optical adjustments. For while in the ordinary mode of viewing an object with the two eyes we adjust both the refractive power and the axial convergence to the same distance, viz, that of the particular point on which for the moment we fix our attention, *in these binocular combinations we are required to adapt the refractive power to the actual distance of the drawings at the same time that we converge the axes to the point behind or in front of them in which the resultant figure appears.* This capacity of the eyes to depart from the usual association of adjustments and to accommodate themselves to the distinct vision of an object in a position nearer or more remote than its true place, has been considered by Prof. Wheatstone in connexion with a peculiar class of experiments with his stereoscope (Phil. Mag., 4 series, vol. iii), but it has not received from writers on physiological optics the attention to which, from its relations to constrained binocular vision in general, it would seem to be entitled.

The mode of union of the two adjustments of refractive power and axial convergence which has been habitual from infancy cannot be interrupted without more or less of effort, and this in the case of persons unpracticed in the experiments above described produces the sensation of a forced condition of the eyes, along with a fluctuating and often confused impression of the binocular image. That these effects are not due to any difficulty attending the axial or refractive adjustments separately considered, is obvious from the fact that we are able with perfect ease to vary the convergency to nearly all distances, and to adjust the refractive power of either eye alone or of both to any point not within the limit of distinct vision. It is therefore to the suspension of our usual association of optical adjustments that I would ascribe the peculiar sense of constraint which at first accompanies the above mentioned experiments in binocular vision.

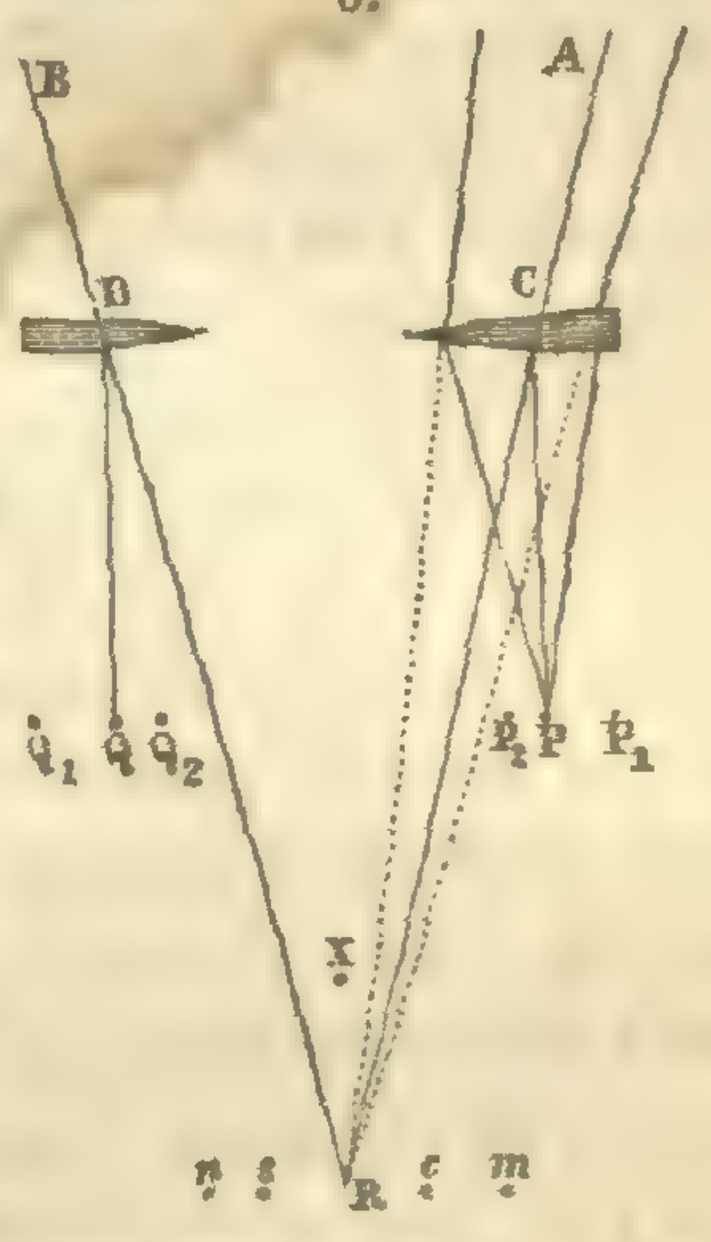
This feeling of constraint so embarrassing to an inexperienced observer ceases after we have learned by a little practice to separate the two optical adjustments as above mentioned. We then find no difficulty in combining the drawings into a precise and clear resultant, and fixing the attention upon it as steadily as upon an actual body in the same position.

5. Of the unusual adjustment of refraction and axial convergence with Brewster's and Wheatstone's Stereoscopes.

The sense of unnatural tension above referred to is not confined to observations with forms of Stereoscope like that just described. It more or less attends the use of all stereoscopes however perfect their construction, or accurate the disposition of the drawings. It may indeed be stated as a general proposition that whenever a perspective resultant is formed by the binocular vision of plane drawings, there are certain parts of the resultant which cannot be seen without a separate adjustment of the refractive and the axial convergence of the eyes. As this condition attending vision with ordinary stereoscopes even in the best arrangement of the drawings, appears not to have attracted the notice of writers on the subject, the following illustrations of it may prove interesting.

In fig. 6 are represented the essential parts of the refracting stereoscope of Brewster now so familiarly known in this country and Europe. P and Q denote corresponding points of the drawings which by the use of the semi-lenses C and D are to be united into one. When these points are properly adjusted as to their distances from C and D and from each other, this union is effected by ordinary unconstrained vision. In this case the conical pencils proceeding from P and Q have their virtual foci coincident at some point R, and the eyes at A and B receive the light exactly as if a real radiant were situated there. But in a different adjustment this coincidence does not take place. Thus if P and Q be moved farther from each other to P and Q, the virtual focus of the former will be shifted from R to *m* and that of the latter from R to *n*. If they be moved nearer together as for example to P_2 and Q_2 their foci will be transferred to *s* and *t*. In the former case *m* is the object proper to the eye at A, and *n* that to the eye at B; in the latter, S is the object proper to A and *t* that to B. Hence while the visual lines for *m* and *n* meet when prolonged at a point V beyond R, those for *s* and *t* intersect one another at a point X nearer than R.

6.

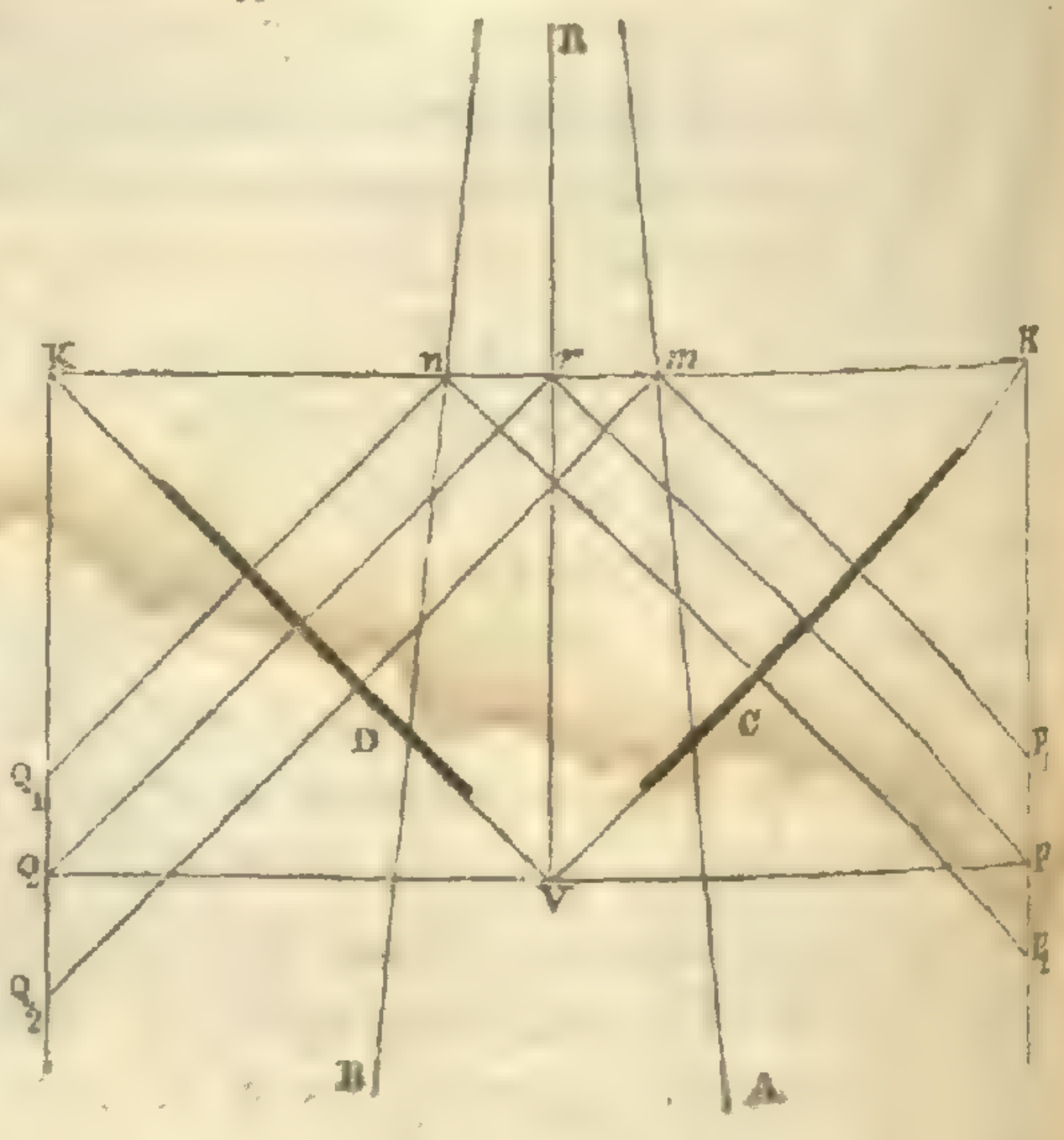


In either of these cases, to combine the two images into a single resultant will require the unusual optical adjustment employed in the experiments with the sliding stage stereoscope. In order to unite *m* and *n* we must make the same separate adjustments of refraction and axial convergence as when using the upper stage and forming the resultant beyond the drawings—and to combine *s* and *t* we must proceed as if using the lower stage and viewing the resultant nearer than the drawings. But it is obvious that

the best practicable adjustment of the lenses and diagrams can only secure the direct optical coincidence of the virtual foci first referred to for certain points or lines of the drawings, and must leave all the rest to be combined by the method of constrained or unusual adjustments of which I have been speaking.

The same thing occurs in using the reflecting stereoscope of Wheatstone (fig. 7), if after adjusting the diagrams so as to cause the right and left images to appear side by side instead of being coincident, we then by constrained vision bring them together so as to form the resultant in a position nearer or more remote than the plane in which they are situated. Thus if *m* and *n* denote the virtual foci or images of P and Q, formed by reflexion from the inclined mirror C and D, these by ordinary vision will appear to the eyes at A and B as separate objects actually placed at *m* and *n*. But we may readily unite them beyond R by converging the optic axes to the proper point while we maintain the refractive power in its previous adjustment to the distance of *m* and *n* from the eyes. Assuming the line VR to bisect the angle of the mirrors and HP KQ to be parallel to VR and equidistant from it, and supposing PQ, to be at right angles to it, it is evident that P and Q, considered as points in the drawings, will have their virtual foci optically coincident at *r*, and will be seen as one in that position. If however we move them backwards on the lateral slides of the stereoscope to PQ, equidistant in the same direction from PQ, their images will be found at *m* and *n*, and so when moved forwards to P_2Q_2, the same distance in front of PQ, their images will take the positions *n* and *m*, the reverse of the former. When however we adjust them in the position P_2 and Q_1, the former as far in advance of P as the latter is behind Q then the two images will be seen optically coincident at *n*, and so if placed at P_1, Q_2 they will form a united image at *m*.

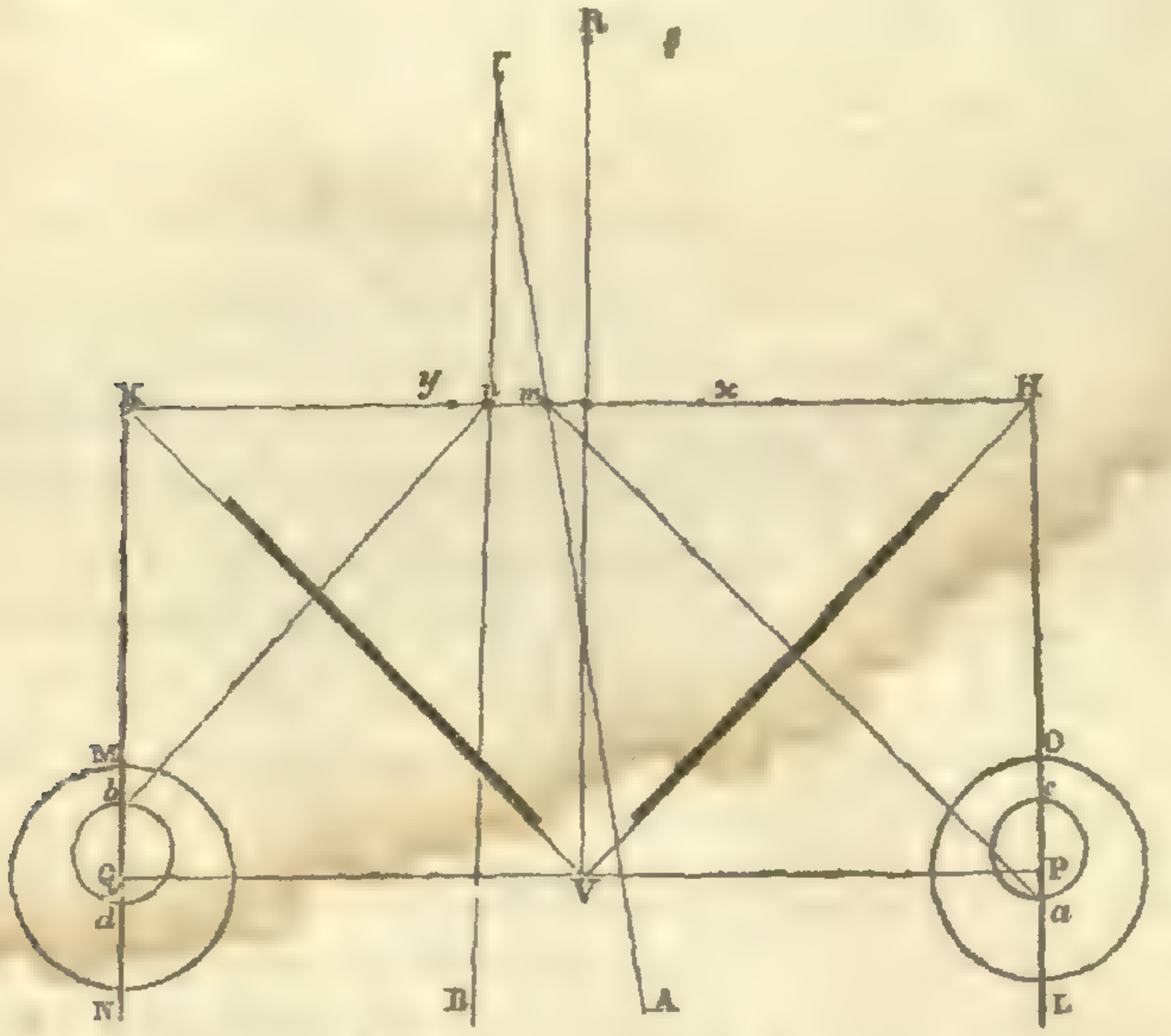
7.



From these relations we draw the general conclusion, that supposing the stereoscopic drawings to be duly adjusted in the slides, the points situated at P and Q and such other pairs of points as are equidistant from PQ in opposite directions will all be united pair by pair in different positions on the line HK; all pairs of points situated in front of PQ or behind it, or on opposite sides

of it but at unequal distances from it, will form separate virtual foci on HK. The former will of course be seen united by the natural process of vision as if the coincident foci were real objects; the latter will appear separate, one to each eye, and will require for their union the convergence of the axes to some point either nearer or more remote than the pair of images, at the same time that the refractive power of the eyes is maintained in its adjustment to the distance at which the images are placed.

8.



To apply these considerations to a particular case in which a perspective resultant is formed, let the drawings placed in the stereoscope (fig. 8) be those which by their union form a hollow truncated cone and let the centres of the two external circles be placed at PQ. In this adjustment the images of L and M will be optically united on HK at a point x on the left of the centre of the line and at a distance equal to the radius of the circle, and the images of N and O will be united at a point y at an equal distance from the middle of HK towards the right. All the other corresponding points of the two exterior circles will be optically united in the vertical plane of HK, so as to form an equal resultant circle in that plane, having its centre in VR. Turning now to the smaller circles it is obvious that the corresponding points a and b form separate images at m and n respectively which in virtue of the forced or unusual adjustment are united at r. In the same way we are presented with the resultant image of c and d at s in a symmetrical position on the opposite side of VR, and thus the resultants of all the corresponding points of the two smaller circles unite to form a circular image in the position rs parallel to and behind the resultant circle at xy. The diameter of this image is greater than that of the component circles ab or cd in the same proportion as its distance from the eyes at AB exceeds that of HK from AB.

It thus appears that in the positions above chosen for the drawings, the combination of the two smaller circles into one in the perspective resultant involves the process of constrained visual adjustment, while that of the larger circles is directly effected by their optical union in the vertical plane of HK. But if both drawings be moved towards HK until L and N are at small distances within P and Q, the eyes at AB will see the images in the vertical plane of HK on opposite sides of the central line and at a distance asunder equal to twice the joint distance of L and N from P and Q. These images will undoubtedly have exactly the same relations to the eyes as the drawings would have if placed on the upper stage of the sliding-stage stereoscope, to be united by converging the axes to points beyond them.

We see therefore, that with the most perfect adjustment of the drawings in a stereoscope, many parts of the resultant pespective figure are formed by a forced union of images that do not optically coincide, and that they become distinctly visible only through a separate adaptation of the refraction and the axial convergence of the eyes. Such indeed is the condition under which we see all parts of the perspective resultant not included in the vertical plane (that of HK fig. 8) in which the images of the drawings would be separately seen.

In describing the use of his beautiful invention, the reflecting stereoscope, Prof. Wheatstone appears in part to have overlooked the considerations above mentioned, for he says (Phil. Mag., 4 series, vol. iii), "the two pictures or rather their reflected images are placed in it at the true concourse of the optic axes, the focal adaptation of the eye preserves its usual adjustment," and again in the same paper, "there is only one position in which the binocular image will be immediately seen single of the proper magnitude and without fatigue to the eyes, because in this position only, the ordinary relations between the magnitude of the pictures on the retina the inclination of the optic axes and the adaptation of the eye to distinct vison at different distances are presumed!" This statement as we have just seen is true only of those parts of the resultant which lie in the vertical plane of HK, but in regard to the rest and by far the greatest part of the perspective figure, the "ordinary relations" mentioned by Prof. Wheatstone are not preserved.

I would remark in conclusion, that this necessity of departing from the usual adjustment of the eyes is no doubt one of the reasons that so large a number of persons having sound and equally balanced eyes, are unable on first looking into a stereoscope, to perceive the resultant figure clearly and steadily, and that even experienced observers are obliged in the case of certain figures to use considerable effort in order to effect the binocular combination.

ART. XI.—*Researches in Magnetisation;* by M. JEROME NICKLÈS.

IN two memoirs, published in the numbers of this Journal for January and March, 1853, treating of the elongation of magnetised bars and of the influence this elongation exerts on the attracting force, I have admitted that the attraction will increase with the distance that separates the two poles of the bar, resting this conclusion on the consideration that in the separation of the poles, the effects of neutralisation which they exert between themselves will be diminished. After giving some proofs to this end, I show that the influence excited at a given time may change its sign, and be reduced to zero, if the electromagnet have a horseshoe shape* so that both poles act on the armature.

These facts have since been verified, and they authorise our inferring what may happen, when, the current or helix being unchanged, we vary the distance between the polar branches of a bifurcate electromagnet. For, on increasing this distance, the mass of the iron of which the electromagnet consists is increased, or, what is the same, the polar branches are elongated, which occasions a tendency to augmentation of force; the chances of neutralisation between the two poles are diminished, whence results another tendency to increase of force.

It is easily seen that the two tendencies are not of the same kind; one may be at zero, when the other exerts its whole effect: by separating to a greater distance the parallel branches, there is no change in the relative situation of the helices or of the branches, while the position of the poles with reference to the armature is altered: for it is evident that the armature intercepts more magnetic rays when the poles are remote than when they are very near.

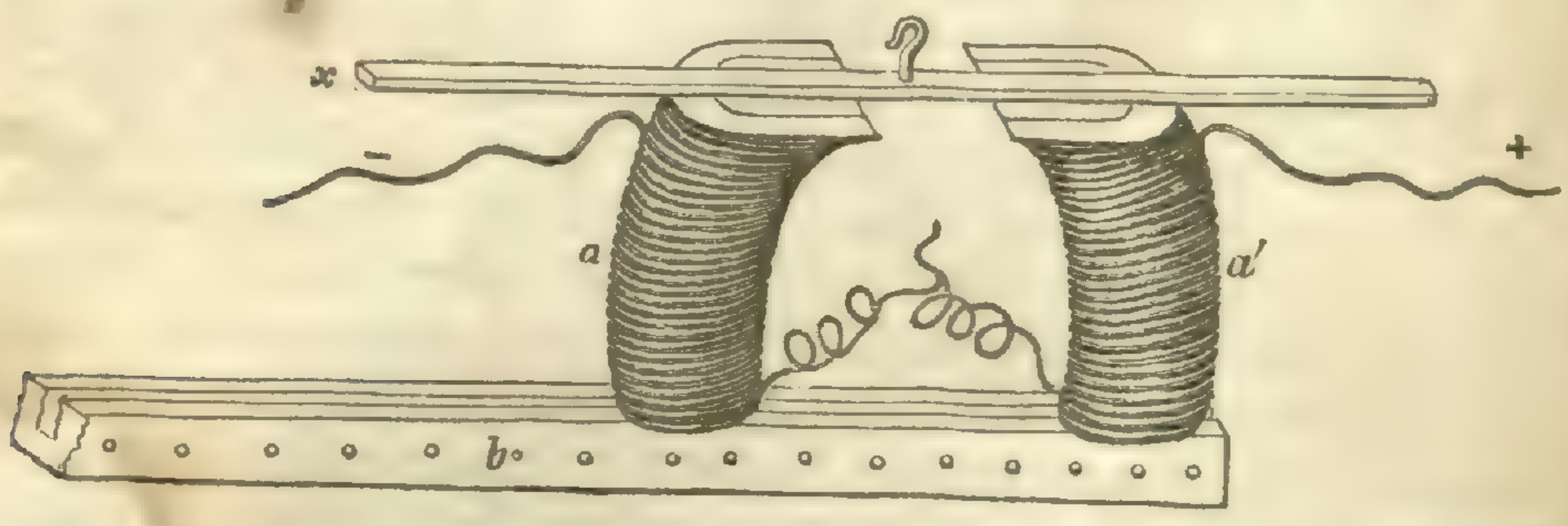
In the course of my researches on circular electromagnets, I have often verified this conclusion, and as the laws of these electromagnets are the same as those which govern bifurcate electromagnets, it may be seen that the separation of the poles will also have some effect on the power of these magnets. All phyicists are not of this opinion, and M. Dub besides others has just pronounced formally the contrary opinion, as a result of experiments whose precision I do not intend to question.

The facts which I have made known, while they do not contradict the results of his observations, weaken the force of his conclusions. To test it most satisfactorily, I operated, as he, with bifurcate electromagnets. The apparatus employed was a

* And not only in electromagnets which are wound with wire through all their length, as M. Dub observes in Poggendorff's Annalen, vol. xc. The same error is contained in the Jahresbericht of Liebig, &c., 1853, p. 248.

horse-shoe magnet (fig. 1), one leg of which is moveable and may be removed at pleasure. The joining piece, by which these two bars are made a horse-shoe magnet, is a rectangular iron bar, of convenient length, having a groove in the direction of the

1.



axis. The fixed leg a' is riveted to one of the extremities of this groove; the mobile leg has a shoulder which fits it to move in this groove; holes are made at intervals in the joining piece, so as to fix by means of a screw the moveable polar branch. The two branches terminate in a common straight line at their lower extremity; the upper extremity is recurved: the radius of the curve is greater than the radius of the helix, so that the two poles may be brought together till they touch by one of their sides if the branches are approached sufficiently near one another.

Perfect contact is not necessary in the demonstration which I would give; besides, as the legs may be indefinitely separated, it is easy to place them in extreme conditions, and so decide the question at a single trial. I have used a constant current, a needle being employed to test it.

Distance between the poles.	Current *a*.	Current *b*.
8 leaves of paper ($\frac{1}{2}$ mm.)	14–15 kil.	52 kil.
120 mm.	18 "	65 "

It is seen that the difference is very apparent. Now it was still to be ascertained whether these numbers express the limits, or whether the results vary with the separation of the poles. It was easy to observe, that with the intensities in play, the attraction no longer sensibly increased on going beyond a distance of 12 centimeters; that the distance favorable for the increase, augmented with the magnetic power developed and diminished when the current itself diminished. This may be seen in the following table which contains some of the mean results, observed at different intensities. I add that the battery used was made of plates of large size; that the branches aa' of the electromagnet were 15 mm. in diameter, and 9 centimeters long; that the helix contained each 47 meters of wire 1 mm. in diameter, and finally that the armature x, was a cylinder of iron 15 mm. thick and 30 centimeters long.

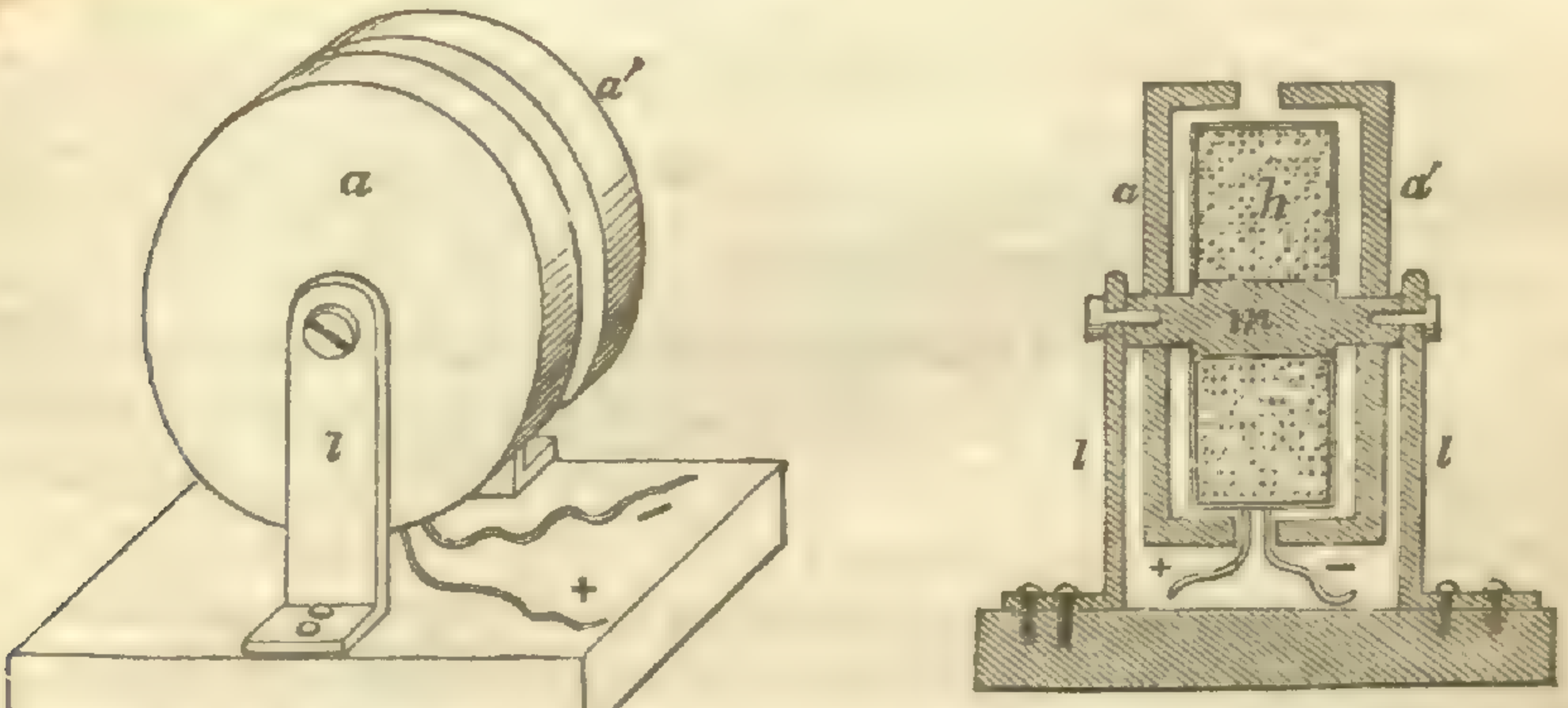
Distance between the poles.	Current *a*.	Current *b*.	Current *c*.	Current *d*.
Thickness of a leaf of paper,	5 kil.	10 kil.	17 kil.	45 kil.
0·0005 meter - -	8	14–15	22	52
0 0·25 - - -	10	16	23	55
0·045 - - -	10	18	25–26	58–59
0·120 - - -	9	18	27	65
0·220 - -	7	18	27	66
0·280 - - -	5	15	27	66

These results establish another analogy between the bifurcate electromagnets and the rectilinear. It is seen that the numbers which express the magnetising power increase at first regularly as in the last;* that they then decrease after passing a stationary point, variable with the intensity of the current or with the magnetism developed, and whose range augments with these intensities.

The residual magnetisms of the electromagnets employed exhibits the same relation after the interruption of this current; the armature falls spontaneously when the poles are at a small distance from one another; it remains suspended when this distance is increased; finally it falls again when the separation has reached a certain point.

Analogous facts have been observed with a circular electromagnet† constructed thus: it consists (fig. 2) of two disks of iron

2.



aa' 9 centimeters in diameter, and 2 in thickness, excavated to a depth of 8 millimeters; these two disks are put on an axis *m*, 35 mm. in diameter; the helix *h* is wound on the middle of this axis, and the helix made narrow so as to be enclosed by the disks; the disks are moveable, and can be either put in contact or removed at will to 15 mm.; they are fixed on a brass support *l*. With this apparatus, the increase of force produced by the separation of the polar circles is so sensible that it is recognisable by a simple touch with an armature in spite of the different causes which tend to vary the effect. The following are some of the results.

* This Jour., xv, 318. † This Jour., xvi, 180.

Dist. between the circles.	Current *a.*	Current *b.*
Contact,	1 kil.	1½ kil.
Thickness of a leaf of paper,	3	5
1 mm.	5	10
2 "	9	12-13
10 "	9	15
14 "	7	15

These facts added to the preceding explain the results obtained by M. Dub, as well as the conclusions which he draws. This physicist, not having sufficiently extended the limit of the separation of the legs of his electromagnet, his limits varying between 2½ and 5¼ inches, obtained nearly invariable numbers, like those which I myself obtained under the same circumstances (Table 2).

Practical Conclusions.—To the conditions required in the construction of straight, bifurcate, or circular, electromagnets, should be added,—giving the poles a remoteness corresponding with the magnetic intensity to be developed; the mean distance for bifurcate electromagnets of the ordinary size may vary between 6 and 12 centimeters, which corresponds to the distance commonly used. It is necessary absolutely to reject an arrangement like that given in the work entitled, *American Electromagnetic Telegraph*, an arrangement which requires that the poles should be brought almost in contact.

ART. XII.—*Correspondence of M. Jerome Nicklès, dated Paris, March* 1, 1855.—(*Continued* from vol. xix, p. 416.)

Annual Session of the Academy of Sciences.—Distribution of Prizes.—This session was opened, as is usual, with discourses and historical eulogiums. The distribution of prizes next followed. The Cuvierian Prize, which is given only for works of the first merit, was presented to M. J. Müller, for his Researches into the Structure and Development of Echinoderms, one of those works "which have contributed most to the philosophy of the science, to organogeny, zoology and general physiology, since the death of Cuvier." This is the second time the Cuvierian Prize has been given, it having been awarded for the first time in 1852 to Agassiz, for his work on Fossil Fishes. A prize of 2000 francs was given by the Section of Medicine to M. Berthelot for his chemical researches on the fatty bodies. Foreign men of science as usual, have received a share in this distribution; among them Wilhelm Boeck and Danielson for their Treatise on the "Elephantiasis des Grecs," and M. Schiff of Frankfort, for his memoir "on the influence of the nerves on the nutrition of bones." Medals were decreed to all the astronomers who have during the year 1854 discovered planets—to MM. Luther, Marth, Hind, Ferguson, Goldschmidt, and Chacornac.

Three awards were given for improvements in the processes used in the Arts that are injurious to health—one for the substitution of potato starch

for wood charcoal in preparing moulds of clay for receiving copper, bronze and melted cast iron, proposed by a poor armorer, M. Rouy ; a plan now generally adopted in the founderies in France, because it is not so hurtful to the workmen, although starch is dearer than charcoal powder. Another award was made to M. Mabru for a process for preserving milk in its natural state, which is simply this—tin canisters having a small tubular opening are filled full and then kept for some time in a water-bath to drive off all the air, and finally hermetically sealed.

Spongy Metals used in therapeutics.—We have already spoken of the spongy metals which M. Chenot has obtained by reducing the oxydized ores by means of a gaseous mixture of hydrogen and of oxyd of carbon produced through the decomposition of steam by incandescent charcoal. M. Chenot now proposes the use of these sponges for cauterising. In a trial which he has made, the blood was immediately coagulated, water absorbed, then decomposed, the oxygen becoming united to the metal and the hydrogen set free : these two effects give place to an energetic local action due to the disengagement of heat produced by the reaction. The application of the sponge of iron modified promptly and beneficially the suppurating parts, and brought about in a short time large and profound ecchymoses, etc.

Telegraphic messages simultaneous in two directions.—Much has been recently said on the communication of telegraphic messages in opposite directions on the same wire at the same time, which was first done the last year by M. Ginth, director of the Austrian telegraph lines. The signals were obtained by an electro-chemical way, the electro-magnetic action not answering in this case.

These facts call to mind what I remarked during the last year on a memoir on the heating of a conducting wire by the passage of two opposite currents according to an experiment made by Provostaye and Desains. I do not consider it a simultaneous passage of electricity in opposite directions in one case any more than in the other. If the electro-chemical telegraph is effective, when the electro-magnetic is in action, it is because the chemical action demands for its exercise less time than that required by the deviation of the needles or the attraction of the electromagnets. It is hence conceivable, that the exceedingly small difference of time which exists between two pulsations at the two extreme stations, these being never absolutely at the same moment, will be sufficient for a chemical impression and not for a mechanical impulse. M. Zantedeschi, of Italy, who has seen the Austrian experiments, observes that not only contrary currents may be propagated ; but also that light and physiological effects may be obtained by opposing two equal induced currents. Experiments of great exactness made by M. Gaugain with Ruhmkorff's apparatus have demonstrated that this cannot be, and that two equal induced currents opposite and synchronous wholly neutralize one another.

Calcium, Barium, Aluminium.—M. Bunsen has sent to the Academy a considerable quantity of metallic calcium which he has prepared by means of the pile. Its color is gray and lustre brilliant ; when heated, it fuses and burns with a bright light. He states that one of his students has prepared barium and strontium in the same way, and he is now studying their properties.

The production of aluminium is encouraged by the French government, who wish to procure a large quantity of this metal in order to use it for cuirasses and other purposes, for which it is especially fitted by its tenacity and lightness. The chemical establishment at Javelle near Paris, has engaged to furnish a certain number of kilograms of aluminium per week.

Pisciculture—The Piscicole Institution at Huningue (Haut-Rhin) distributed last year (1854) several thousands of fertile eggs of salmon, trout, &c. A large part of them were sent to the establishments founded on the model of that of Huningue, in France, England, Germany, Switzerland, etc. MM. Edmond and Thomas Ashworth of Ireland received eggs from Huningue for the establishment at Long Corrib, Ireland; and in 1854, 250,000 salmon were contained in their enclosures. M. Ramsbottom, under their direction, has established 350,000 on the borders of the river Tay, and M. Ayrton, as many more on the Dee.

The French government continues to sustain the establishment at Huningue. They make it also a school, where those who wish to learn the methods of manipulation may enter as pupils.

Correspondence of M. J. Nicklès, dated May 2, 1855.

Necrology.—The scientific world has of late suffered greatly by the loss of eminent men—Mathematics in the person of M. Gauss, and Natural Science by the death of Duvernoy, De la Beche and Greenough.

M. Gauss died on the 23d of February, 1855, at the age of 78 years. He was Free Academician of the Academy of Sciences at Paris, succeeding to the place of Sir Jos. Banks. M. Gauss represented in Germany what is known in France as the school of Laplace, that is to say, the metaphysical department of pure mathematics.

M. Gauss never shrank from any difficulty in the sciences. His theorem relative to the method of least squares, for arriving at the most probable or most correct of several slightly differing results—that for the motions of comets and of planets with their perturbations—that of the flexure and elasticity of surfaces forcibly moved from their primitive condition—and his algebraic labors in general, have all given him a place of the first rank in mathematical science.

His theory of the absolute value of the magnetic force of the terrestrial globe and the applications which have been made of it have enabled us to grasp clearly one of the elements of our spheroid. His theoretical discussion upon the division of the circle and on regular polygons, as well as his arithmetical studies upon the theory of numbers are regarded among the most abstract conceptions with which the human mind can be exercised.

Conceiving himself unknown or unappreciated, Gauss had withdrawn from the world for many years past: nevertheless his contemporaries were filled with admiration of his genius, although few among them were capable of reading his works understandingly and consequently of fully appreciating them.

Possibly it was for the purpose of making himself accessible to a greater number that Gauss took up the investigation of the Electric

Telegraph and from 1833 to 1836 he made, in connection with his pupil, Wilhelm Weber, the earliest useful experiments in Electro-telegraphing, in which he employed a battery, a magnetic needle, and a wire connecting the University of Göttingen with the Observatory.

L. G. DUVERNOY deceased on the 1st of March, 1855, was born at Montbéliard (department of Doubs) the 6th of August, 1777. Till the age of fifteen he remained with his parents and received his early education under the eye of his father. He then went to Stuttgard, where he remained only a short time, because the province of Montbéliard having been added to the French Republic in October, 1793, young Duvernoy was required to return to France under the penalty of being considered as having emigrated. He continued his studies at the Academy of Strasburg, then filled with the recollections of the preceding generation in which were Cuvier, Goethe, Börne, the linguists Brunck, Schweighäuser, etc., and where he had for his fellow pupil the chemist Braconnot. He there pursued the study of the medical and natural sciences and had already made great progress in them when in 1799 he was called into active military service, entering as military physician in the army of the Alps, the left wing of the army of Italy. An epidemic typhus which broke out in the army gave him an opportunity to distinguish himself by his courage and devotion. The distinguished Parmentier, struck by the high courage and spirit of the youthful Duvernoy, wished to withdraw him from the hazards of war and obtained for him his discharge the following year. The earliest published works of this indefatigable laborer date from this time, his latest memoir was finished only one month before his death. The interest which so many distinguished men then felt in him was justified by several memoirs published in the journals of the day, the most remarkable of which appeared in 1799, under the title of "*Reflexions sur les corps organisés et les sciences qui en sont l'objèt.*" On his return to Paris he finished his studies and achieved his medical diploma by his thesis "*Sur l'hystérie.*"

Here begins the real scientific life of Duvernoy, with the date of his connection with that illustrious man who lived to found the sciences of Comparative Anatomy and Palæontology. Cuvier, who was his relative, recalled him to Paris in 1802, to aid him in numerous scientific labors, and having recognised his powers and his exactness as an observer, associated him with himself in his great work "*Leçons d'Anatomie comparée.*" This was in 1803. Cuvier had just published with M. Dumeril the two first volumes of this fine work. These volumes comprise the description of the skeleton, the organs of motion in general, the nervous system, and the organs of sense. To fill out this growing science, Cuvier took M. Duvernoy as his associate. Cuvier himself wrote out the general views which precede the chapters of descriptions in this work, as well as several of the latter, for example, that on *the teeth.* The rest was the work of our young anatomist, then twenty-six years of age. Having at command the rich collection which Cuvier had destined for this gigantic work, he dissected constantly, prepared his descriptions, his master only reviewing the proof-sheets. Cuvier often mentioned the value of the aid given him; he thus says in volume iii. of the "*Le-*

çons," "j'avoue cet'ouvrage comme le mien, tout en reconnaissant qu'il appartient aussi à M. Duvernoy, non seulement par la rédaction mais aussi par beaucoup de faits curieux dont je lui dois la connaissance." The last three volumes were published in the space of two years. This great labor did not hinder M. Duvernoy from publishing during this period nine memoirs on different subjects of physiology, of pathology and comparative anatomy. Such labors demanded a reward which Cuvier was impatient to obtain for his kinsman. In this he succeeded in 1809, when M. Duvernoy was named Assistant Professor of Zoology to the Faculty of Sciences in Paris, a chair then established. Cuvier hastened to recall to himself the young *savant* who having married in 1803, had returned, after the publication of the Leçons, to Montbéliard where he practised medicine. Duvernoy returned, to receive from Cuvier the highest possible mark of his confidence. Forced to absent himself, the great naturalist confided to M. Duvernoy the direction of all his scientific affairs.

The future seemed to open to M. Duvernoy a career of increasing celebrity: nevertheless he abandoned all, and again retired to honorable and laborious obscurity. His wife and children had remained in his native place. A deep sadness seriously affected his health, and on the 30th of December, 1809, he abruptly left Paris, abandoning a future of renown to find again his life and happiness in the joys of his family. Thus closed for a season the first period of the scientific life of M. Duvernoy. Devoted to the fatigues of medical practice, he toiled more than twenty years over the mountains of his native country to save and console the sick. His name rests in honor among the inhabitants there, and thither his mortal remains were borne. Duvernoy appeared then lost to the scientific world. Descriptions of fossil bones came at long intervals to call to mind the former labors of the physician of Montbéliard; but until 1827 it was feared that he was forever estranged from the field of scientific labor. About this period, at the age of 50 years, he reappeared broken by sorrows and seeking from science consolations for his misfortunes. His wife and several of his children had died in his arms. Happily, two of his daughters remained who with their second mother received all his affections and consoled his old age to the end of life.

In 1827 he accepted the chair of Zoology in the Faculty of Sciences at Strasburg and thus entered upon the second part of his scientific career. This had little resemblance to the first. Driven, contrary to his expectation, into an arena where new competitors had arisen, he was destined to meet new obstacles at every step. This only made him confident and more ardent in labor. Returning, from his retreat, to a science of which he had twenty-two years before laid the foundations, he found it enriched by new facts which he rapidly appropriated; and he prescribed for himself the mission of completing his unfinished labors of 1802. Cuvier still lived: but without ceasing to be the admirer of the great naturalist, Duvernoy remained free and independent, and honored his master by avowing, without fear of displeasing him, scientific opinions differing from his own.

Never was there a scientific life of greater labor than that of Duvernoy during these last twenty-seven years. They were divided into two peri-

ods, the first of which he passed at Strasburg and the second, which terminated with his life, in Paris. Before 1827, Duvernoy had never instructed, and he began the labors of his Professorial Chair with a review of the progress of natural history in the first twenty-five years of this century, and thus resumed his labors which were never again interrupted. The course with which he opened his career at Strasburg served as the point of departure for his original researches. In 1828 he remodeled, according to his own ideas, the classification of mammals, while in a series of 27 memoirs continued down to 1838, he described a crowd of anatomical facts, and cleared up many obscure points in Physiology. To this period belong his researches "Sur les serpents venimeux;" the monograph "des Musaraignes"; his studies "Sur le foie," the discovery "Des cœurs accessoires de la Chimère arctique;" "De l'organisation du cœur des Crocodiliens," &c. In 1832, Cuvier having died of an attack of cholera, Duvernoy sought to succeed him in his chair at the *Muséum d'Histoire Naturelle* at Paris but failed through a small deficiency in the ballot. The succession of Cuvier devolved on M. De Blainville, and M. Duvernoy achieved this honor more than 17 years later, upon the decease of this naturalist. This first check commenced the series of disappointments by which Duvernoy was to expiate his absence of twenty years. But he very soon gave his attention to the completion of his great work. Cuvier had left incomplete a second edition of his Comparative Anatomy. Duvernoy in 1837 commenced the revision of the part which he had edited in 1805. He made it a new work; the descriptions were verified from nature, the observations of other naturalists were severely scrutinized, and his own discoveries came in to enrich the facts already known. This work in five volumes was finished in 1846. Nominated Dean of the Faculty of Sciences at Strasburg in 1833, he did not resign these honorable duties until 1837 when he was entrusted with the chair of Natural History in the College of France. He was replaced by Dr. Lereboullet in the chair of Strasburg, who still holds it with distinction. His duties at the College of France commenced in 1838 and continued without interruption until 1850. The several branches of zoology were there passed in review. Finally in his later years he undertook a complete revision of zoological classification and proposed a method entirely his own. He hoped to put in print this last fruit of all his scientific studies, but death intervened to prevent. In the period from 1838 to 1850 he produced sixty-five memoirs consecrated particularly to anatomy and physiology. The memoirs upon the teeth of the Musaraignes (genus, Sorex) alone occupied eight years of persevering labor. In them he broached a new theory of the development of teeth.

M. Duvernoy was in succession, Correspondent of the Institute, and Free Academician; he occupied the chair of Cuvier at the *College of France* and at the *Muséum d'Histoire Naturelle*. On the 4th of December, 1854, he was seized with an attack of suffocating bronchitis. Escaping this first blow he sought to resume his occupations, but the disease made progress and he well understood that life was over with him. He assembled around him his family and his dearest friends to bid them farewell and leave them his love. M. Duvernoy desired to be interred at Montbéliard, his native city. Dumeril, the zoologist, the

companion of his youth, accompanied his remains even to their destination notwithstanding his great age and the severity of the season.

Magnetic force of Oxygen.—In 1849 M. Edmond Becquerel, Professor at the "Conservatoire des Arts et Metiers" in studying the action of electro-magnets upon various bodies, established that oxygen is magnetic and that atmospheric air in virtue of the oxygen it contains, partakes of the same property. The mode of experiment to measure the force exercised by a magnet on a gas in comparison with the effect produced upon a body taken as unity, consisted in placing successively small bars of glass, wax, &c., in a vacuum and in different gases, in order to determine the magnetic power of the gas by the difference of effect produced under these two conditions. He thus established that the relation between the attraction of oxygen by a magnet, and the repulsion in an equal volume of water, is proportioned to the density of the gas; and that it is represented by 0·18 at the temperature of 12° C.

If we reflect that the earth is surrounded by a mass of air equivalent in weight to a stratum of mercury 76 centimeters in height, we can understand that a similar mass submitted to the incessant variations of pressure and temperature, ought to exercise an influence on some of the phenomena dependent on terrestrial magnetism. In calculating what is the real magnetic power of this fluid mass we find that it is equivalent to an immense shell of iron of a thickness of $\frac{1}{10}$th of a millimeter, covering the entire surface of the globe. M. Becquerel's rerulis have been confirmed by M. Faraday* and by M. Matteucci by different methods. M. Plucker having reached other results by the use of a method of his own—a process by weight—Becquerel has revised his researches and confirmed anew the results previously announced.

Oxygen in the nascent state—Ozone.—For some time past, observations have been made in Europe on atmospheric ozone. Owing to the persevering efforts of MM. Wolf of Berne, D. Bœckel of Strasburg, and Dr. Simonin of Nancy, some general facts of the highest interest have been reached by the use of Schönbein's test (i. e. paper made sensitive by starch and iodid of potassium). According to these observations there exists an intimate relation between the quantity of ozone in the air and certain epidemic diseases such as cholera, grippe, intermittent fever, &c. They think they have established that the appearance of the grippe coincides with the presence in the air of an excess of ozone: that on the contrary the invasion of cholera is accompanied by an almost complete absence of ozone in the air; this is at least true for the places above named.†

It is well known that ozone is regarded as an isomeric or allotropic condition of oxygen. MM. Becquerel and Frémy have called it electrized oxygen and have prepared it, as we have before described, by submitting pure oxygen to the electrical current.

The following is a new mode of preparing it in abundance (or at least a *similar* body) capable of oxydizing silver, of decomposing iodid

* Faraday's results were published before 1849, in 1847, his first paper on the magnetism of oxygen having been read before the Royal Society in that year, and published in the Philosophical Magazine for Dec. 1847.—Eds.

† It will be remembered that in 1849 much was said in the medical journals of this land, on the relation between cholera and ozone, the absence of ozone being insisted on at that time in cholera districts in the United States.—Eds.

of potassium, of burning ammonia, of disengaging chlorine from hydrochloric acid, and of forming water with hydrogen. This simple process consists in treating peroxyd of barium (BaO_2) with monohydrated sulphuric acid at a temperature below 70° C. The oxygen disengaged in this process possesses the properties named above, and it has the characteristic odor which is known as the Lobster odor. M. Houzeau assistant to M. Boussingault, is the author of this process which he discovered during a series of researches on the preparation of oxygen from the peroxyd of barium by heat.

Attempts to insulate Fluorine.—M. Frémy has been for many years engaged in this research, employing as the means of decomposition the galvanic pile, and selecting for his experiments the fluorid of potassium obtained by the calcination of the fluohydrated fluorid of the same base ($FlH+FlK$) in a platinum crucible. The resulting fluorid is of difficult fusion and in order to submit it to the action of the pile, M. Frémy introduces it into a tubulated retort of platinum which is then submitted to the heat of a good forge fire. A platinum wire communicates the currents from the positive pole and enters the fused fluorid; the negative pole connects with the retort. The platinum reddens rapidly under this action, and is changed to fluorid of platinum which at that temperature is at once decomposed. From the neck of the retort a gas issues, which is strongly odorous; it decomposes water producing fluohydric acid and displaces the iodine of the iodids. M. Frémy regards this gas as fluorine, and he has already prepared it many times in his laboratory at the Polytechnic School. It is in other respects identical with what has been previously obtained by the decomposition of certain fluorids by oxygen at a high temperature.

Aluminium, Silicium, &c. &c.—The experiments alluded to in my last communication are continued with zeal in the hope of producing aluminium in an economical manner. They have commenced by cheapening the production of sodium the present selling price of which in the large way has been reduced to 10 francs the kilogramme, (about 90 c. per lb.). We have already described in a preceding number the improvements which M. Deville has introduced into this manufacture.

Spongy Metals.—We have several times spoken of spongy metals and especially of the spongy iron of M. Chenot who first recognised the importance of employing aluminium as an alloy. He has for many years prepared specimens of very hard steel composed entirely of iron and aluminium—and which have received high commendation from most manufacturers. M. Chenot has often remarked, that in compressing spongy iron, the production of a harsh sound accompanied the rupture of the moulds employed. Very recently in compressing spongy silicium this fact was reproduced in a very marked manner. Three grammes of silicium in the spongy state having been submitted to a pressure equal to 300 atmospheres, it exploded with a fearful noise—the fragments of steel from the broken matrix entered many millimetres into a plate of cast iron, and the body of the hydraulic press which was 20 centimeters in thickness was broken, and this, although the safety valve was open, thus showing the sudden violence of the shock. The action was entirely from above downward since no portion of the upper part of the compressed metal in this case suffered.

This phenomenon has analogies with the fact published by Mr. Gore in the Philosophical Magazine, namely, the explosive property of antimony when loosely arranged to favor its combination with chlorine under the influence of the pile. These facts open to us a new property of metallic substances, due to a particular mechanical state or condition. Possibly it is to this cause that we must refer the explosion so often noticed in the manufacture of potassium and sodium, the cause of which has never been explained, although chemists have frequently sought for it. Recalling the power of spongy platinum in the condensation of gases, we are led to enquire, if the phenomenon noticed by M. Chenot may not be of the same order. The spongy silicium previous to compression had been exposed to air, condensing in its pores a large volume of gas. The strong pressure, to which it was afterwards submitted, suddenly set at liberty the gas by diminishing the capacity of the pores, producing an explosive effect analogous to the sudden evolution of steam from water in the spheroidal state.

Paris Universal Exhibition.—The postponement of the opening of the French Exhibition to May 15, and then to June 1, has long been a recorded fact. It is not our place to describe what is going on in this great cosmopolitan palace, where nations are displaying their chefs d'œuvre. English astronomy will send forward a full size model of the great Greenwich circle executed under the direction of M. Airy at the expense of the British government. The telescopes of Lord Rosse, of M. Lassell, and of M. Nasmyth, will be represented also by models. Mr. Piazzi Smith will send certain astronomical instruments. The new observatory will transmit its complete collection of magnetic and meteorological instruments which will be mounted and placed in action at Paris under the direction of Mr. Welch. The Bureau of Ordnance will exhibit the great theodolite which it has employed in triangulation, and examples of charts on different scales. The department of geological charts has also prepared a collection which will give a complete idea of its labors. The men of science in England at first felt some repugnance to exposing their physical instruments, like manufacturers, but this feeling has given way, and the number of exhibitors in this class will be a hundred or more, among whom appear, MM. Herschel, Brewster, Sabine, James, Willis, Lord Rosse, Wrottesly, Snow Harris, Wheatstone, Lassell, Grove, Warren de la Rue, Arnott, Gassiot, Brodie, and Frankland. Many of the great institutions figure also in the list, viz., University College, Guy's Hospital, &c. Every facility is offered for the exhibition of apparatus and machines in electricity which have not yet received a definite application. While the electric telegraph and telegraphic apparatus will be very severely scrutinized, being regarded as a matured subject, they will be extremely indulgent to machines for electrical motion, as this application stands in need of encouragement. Among machines of this class, will appear the motor apparatus described in this Journal, 1853,* consisting of 4 equal electromagnets acting in succession upon 6 cylinderical armatures placed around the magnets which receive the current alternately by means of four circuit breakers: this apparatus is in active use in a machine shop in Paris, No. 10 Rue

* July, 1853, p. 110.

Bichat. A celebrated juggler, Robert Houdin, will exhibit an electrical pendulum in which he has surmounted two important difficulties, to wit, the disturbing influence of variations of current upon the motion of the pendulum, and the destructive influence of contact breakers. The construction of this pendulum has given to horology a new mechanism which will have important applications.

Photography.—Employment of the Cyanid of Iodine.—M. Stéphane Geoffray, Advocate at Roanne, and a great amateur in the art of photography, has employed with success the cyanid of iodine as the sensitive agent in direct positives. This compound he obtains by the action of iodine upon cyanid of mercury. It is very soluble in a solution prepared with wax and benzine, and gives to this coating a rapidity of action nearly equal to that obtained by the use of ceroleine. Applied to collodion in the proportion of other iodids, the cyanid uniformly affords direct positive proofs of great beauty and which do not subsequently change. After fixation in the old bath of hyposulphite, the lights become very beautiful and are not inferior to those given by the use of the sesquichlorid of mercury.

M. Geoffray also employs the sesquichlorid of iron for all the purposes for which the sesquichlorid of mercury has been hitherto used, to bring out negative proofs on collodion and upon albumen in the manner employed for direct positives. He also employs this agent in place of the iodid to prepare sensitive papers for making positives in the shade and in a few seconds; according to the process of M. Blanquart-Ewrard. With this chlorid he also prepares a dry collodion of excellent quality, as follows:

To 100 grammes of ordinary non-sensitive collodion he adds 50 centigrammes of dry and finely pulverized perchlorid of iron, having no acid reaction; he boils it for a quarter of an hour, and adds four drops of tincture of iodine and filters the mixture. The glass being perfectly cleaned, he pours on the collodion, waits a moment to allow the coating to acquire a certain solidity—then plunges it in a bath of nitrate of silver, again in distilled water, and finally is dried, protected from the dust. The image is revealed as usual by pyrogallic acid.

This collodion is more sensitive than that prepared by using the protochlorids, but is much less so than the moist collodions.

Artificial Alcohol.—In my last communication I neglected to speak of the production of alcohol by means of water and illuminating gas. M. Berthelot has reached this important result through a species of contact between $C_4\ H_4$ dissolved in fuming sulphuric acid and water contained in the acid. This important discovery has been the subject of a Report to the Academy of Sciences by M. Thénard. In this Report (a very flattering one to the young chemist) the venerable Dean of French chemists points out several other attempts of M. Berthelot and among them that of converting grape sugar into cane sugar. In spite of certain difficulties, we may still believe this a possible result. Nevertheless M. Biot doubts the possibility of this change, because it requires that the intimate molecular structure of the substance should be changed, a change to which we have no analogy in the transformations hitherto made known. This difficulty however does not appear to MM. Thénard and Dumas as insurmountable; since in treating cane sugar

with an acid, its molecular constitution is changed so that its rotatory power over the polarized ray is reversed from right to left, why then should it be impossible to convert left-handed rotaton to right-handed?

Late changes in the scientific corps at Paris.—A considerable movement has taken place among the scientific men living in Paris—or who pass the larger part of their time in that city.

FAYE the astronomer and member of the Institute has been appointed Provost (recteur) to the Faculty of Sciences at Nancy. M. MALAGUTI has been made Dean of the Faculty of Sciences at Rennes. M. PASTEUR has been made Dean of the Faculty of Sciences at Lille. M. GODRON, the author of the "*Flore de France*," has been nominated Dean of the Faculty of Sciences at Nancy. M. MORREN has been made Dean of the Faculty of Sciences at Marseilles. M. DESAINS has been appointed Professor of Physics to the Faculty of Sciences at Paris. M. FOUCAULT has been nominated as Physicist to the Paris Observatory. M. LECOQ is made Professor of Botany at Clermont. M. LALLEMAND to be Professor of Physics at Rennes. M. FAVRE to be Professor of Chemistry at Marseilles. M. GERHARDT to be Professor of Chemistry at Strasburg. I may also add that the writer has been appointed Professor of Chemistry to the faculty of sciences at Nancy.

Bibliography.—Etudes physiologiques sur les animalcules des infusions végétales, comparés aux organes élémentaires des végétaux par M. PAUL LAURENT, professor à l'Ecole forestière de Nancy; grand in 4°, orné de 22 planches dessinèes et lithographiées par l'auteur. New York, chez H. Bailliere.—The first volume of this work is now ready. This book contains the fruits of twenty years assiduous labor, and throws entirely new light upon the animalcules contained in vegetable infusions (infusoria); their origin and development and their habits are here exhibited in the most interesting manner. Through new methods (which he describes with care in his work), M. P. Laurent has been able to observe better than his predecessors, to group facts and phenomena which had previously escaped notice, and to reunite in one species individuals which have before been regarded as wholly distinct. This first volume has made a great sensation among micrographers, unhappily rare in France. The second and last volume will not be less curious.

Mecanique analytique, de Lagrange. 3d edition revised, corrected and annotated, by M. Bertrand; t. ii, Paris, Mallet-Bachelier.—We have already announced the publication of the first volume of this work. The second volume treats of Dynamics. It is concluded by several fragments either by Lagrange himself or his contemporaries.

Œuvres Complétes de Fr. Arago, t. iv, containing the historical Eulogies of Malus, Gay-Lussac: the biographies of Copernicus, Tycho Brahe, Galileo, Newton, Kepler, Huyghens, Cassini, Lacaille, Roemer and a great number of other astronomers and physicists.

SCIENTIFIC INTELLIGENCE.

I. Physics.

1. *On the Expansion of certain substances by Cold;* by W. J. Macquorn Rankine, Civil Engineer, F.R.SS., Lond. and Edinb. &c. (Phil. Mag., [4], viii, 357.)—During the discussion which followed the reading of the Rev. Prof. Powell's Report on Radiant Heat, at the meeting of the British Association in 1854, it appears, from the report in the *Athenæum*, that a question was proposed by Dr. Stevelly as to the means of reconciling the dynamical theory of heat with the fact of the expansion of water, antimony, cast iron, and bismuth, by cold, at and near their freezing points; and that some observations were made in reply by Professor Powell and Prof. William Thomson.

The question is one of much importance, and calls for the attention of every one who has been instrumental in maintaining the dynamical theory of heat. Having been prevented by illness from attending the meeting of the Association, I beg leave to offer the following remarks for publication in the Philosophical Magazine.

The theory of thermo-dynamics, strictly speaking, is a system of propositions, all of which are deducible from the following two laws:

I. Heat is convertible with mechanical power.

II. The mechanical equivalent of the quantity of heat which disappears or appears during any indefinitely small change (dV) of the volume of a given mass of a given substance, at a constant absolute temperature (τ), is the product of the following three quantities: the variation of volume, the absolute temperature, and the rate of variation of the expansive pressure (P) with temperature at constant volume; that is to say,

$$\tau \frac{dP}{d\tau} dV.$$

When this product is positive, it represents heat which disappears; when negative, heat which appears.

Neither of those laws indicates any particular relation between the pressure, volume, and temperature of a given mass of a given substance as more probable than any other. The nature of such relations must be determined for each substance by experiment, before the two general laws above stated can be applied to it.

Hence it appears that it is not the province of the theory of thermo-dynamics to explain the fact of the expansion of certain substances with cold, but simply to deduce the consequences of that fact, so far as they relate to the quantities of heat which are made to appear and disappear by effecting given changes in the volume of those substances.*

It is true that if we frame a *hypothesis* as to the molecular structure of matter (such as the hypothesis of molecular vortices), so as to deduce the two laws of thermo-dynamics from those of ordinary mechanics, that hypothesis must lead to many consequences besides those two laws; and it is necessary that those consequences should not be inconsistent with any of the phenomena of the relations between the tem-

* One of those consequences was anticipated by Mr. James Thomson, and proved experimentally by Professor William Thomson, at a time when the theory of thermo-dynamics was in a very imperfect state, viz., that the freezing-point of water is lowered by pressure.

perature, volume, and elasticity of bodies. No such inconsistency has hitherto been proved; but even were such inconsistencies to be proved, no objection to a molecular hypothesis can affect the certainty of the two laws of thermo-dynamics, which, though at first anticipated as the results of a hypothesis, have now been independently established by experiment.

With respect to the molecular mechanism that may be supposed to give rise to the expansion of certain liquids in cooling, near their freezing-points, it appears to me, that so far as our present knowledge enables us to judge, the most probable view is one concurred in by Professor Thomson, viz., that in a liquid near its freezing-point, there is an incipient tendency of the particles to assume a definite arrangement with respect to certain fixed axes; and that in substances which expand in freezing, that arrangement is such as to make the particles occupy more space than they do in the perfectly liquid state.

2. *On the Nature of the Force by which Bodies are repelled from the Poles of a Magnet; preceded by an Account of some Experiments on Molecular Influences*; by JOHN TYNDALL, Ph.D., F.R.S., (L. E. and D. Phil. Mag., [4], ix, 387.)—The paper commences with an introduction, in which the present aspect of the portions of science to which it refers is briefly sketched. A section is devoted to the examination of the magnetic properties of wood, which substance, the author finds, except where extraneous impurities are present, to be always diamagnetic, and to set in the magnetic field with its fibre equatorial. The influence of the shape of pointed and flat poles is studied, and those curious phenomena of rotation, first observed by M. Plücker, and attributed by him to the action of two conflicting forces, are referred to molecular structure as a cause. Between flat poles it is proved that the line joining the centres of the two poles is the line of *minimum* force; that is to say, the force increases more quickly from the central point of the magnetic field in an equatorial direction than in an axial one. Reflecting on the great diversity of opinion at present existing with regard to the real nature of the diamagnetic force, the author deemed it necessary to commence at the foundation of the inquiry. A fundamental question in the present case is the following:—Are diamagnetic bodies repelled by a magnet in virtue of any constant property possessed by the mass? Is the force in question a mere repulsion of ordinary matter, or is the repulsion exercised in virtue of a state of magnetisation into which the body is first thrown? This question is answered in a manner which admits of no doubt.

It is proved that the repulsion of diamagnetic bodies increases in a quicker ratio than the strength of the magnet which produces the repulsion. Within wide limits, indeed, the repulsion, instead of being simply proportional to the strength of the magnet, is proportional to the square of the strength, which leads inevitably to the conclusion that the body thus repelled contributes to the effect produced; that its repulsion is due to an excited condition into which it is thrown by the influencing magnet, the intensity of this excitement varying within the limits already referred to, as the strength of the magnet which produces it. This conclusion is further arrived at by a close comparison of the repulsions of diamagnetic bodies with the attraction of paramagnetic ones: both are found subservient to one and the same law.

It is next proved that the diamagnetic excitement produced by one pole of a magnet is not the state which enables a pole of an opposite quality to repel the substance :—that each pole induces a condition peculiar to itself, or, in other words, that the excitement of diamagnetic bodies in the magnetic field is of a *dual* character.

These points being established, a searching comparison is instituted between the phenomena exhibited by paramagnetic and diamagnetic bodies in three distinct cases :—first, when operated on by the magnet alone ; secondly, when operated on by the current alone ; and, thirdly, when operated on by the magnet and current combined. A bar of iron was, in some of these cases, compared with a bar of bismuth, but it was soon found necessary, in order to avoid the proved errors of reasoning, to take strict account of the molecular structure of the bismuth. A bar of this substance, cut in a certain manner from the crystallized mass, exhibits between the poles of a magnet precisely the same visible deportment as a bar of iron, while it is well known that the normal deportment of bismuth is opposed to that of iron. The author, in his examination of the points before us, divided paramagnetic bars into two distinct classes, and classified diamagnetic bars in the same manner ; one class he called *normal*, and the other class *abnormal*. A normal paramagnetic bar is one which sets its length from pole to pole in the magnetic field, and a normal diamagnetic bar is one which sets its length at right angles to the line joining the poles. An abnormal magnetic bar, on the contrary, is one which sets its length equatorially ; while an abnormal diamagnetic bar is one which sets its length axially.

In all cases, whether operated on by the magnet alone, the current alone, or the magnet and current combined, the deportment of the normal paramagnetic bar is precisely antithetical to that of the normal diamagnetic one. In the magnetic field the former sets axially, the latter equatorially. Operated on by a voltaic current, the former sets its length at right angles to the current, the latter sets its length parallel to it. When magnet and current act together on the bars, it is found that the disposition of forces which produces a deflection from right to left of the paramagnetic bar produces a deflection from left to right of the diamagnetic bar. If the position of equilibrium of the former be from N. E. to S. W., the position of equilibrium of the latter is from N. W. to S. E. In short, the position of rest for the normal magnetic bar is always at right angles to the position of the diamagnetic bar. A precisely similar antithesis is observed when we compare the abnormal paramagnetic bar with the abnormal diamagnetic one. The former, in the magnetic field, sets equatorially, the latter axially. The former sets parallel to an electric current, the latter perpendicular to the same. If the deflection of the former be from right to left, the deflection of the latter, under like conditions, is from left to right. Finally, the position of equilibrium of the former is always at right angles to that of the latter.

But if the deportment of the normal paramagnetic bar be compared with that of the abnormal diamagnetic one, it will be found that they are in all cases identical ; and the same identity of deportment is exhibited when the abnormal paramagnetic bar is compared with the normal diamagnetic one. The necessity of paying attention to structure in exper-

iments of this nature, could not, it is imagined, be more strikingly exhibited.

It is proved by these experiments that the simple substitution of an attractive force for a repulsive one would completely convert the phenomena exhibited by paramagnetic bodies into those exhibited by diamagnetic ones; that if that which Gauss has called the ideal distribution of magnetism in an iron bar be reversed, we have a distribution which would produce the phenomena of a bismuth bar of the same dimensions. All the phenomena of diamagnetic bodies become equally intelligible with those of paramagnetic ones, when we assume that the former class possess a polarity the same in kind, but the opposite in direction, to that of the latter.

It is well known that a bar of iron surrounded by a helix in which a voltaic current circulates is converted into a magnet, and exhibits that *twoness* of action,—those phenomena of attraction and repulsion at its two ends—to which we give the name of polarity. The present paper contains an account of experiments made with the view of ascertaining whether similar phenomena were exhibited by a bar of pure bismuth. A cylinder of the latter substance, $6\frac{1}{2}$ inches long and 0·4 of an inch in diameter, was suspended by a suitable device within a helix of covered copper wire, so that it could vibrate freely from side to side. The ends of two electro-magnetic cores were brought to bear upon the two ends of the bismuth bar, and it was so arranged that the two magnetic poles acting upon the bar might be of the same or of opposite qualities. The helix being first excited by a strong current, a current of considerably less power was sent round the electro-magnetic cores, and their action upon the bismuth bar was observed: by means of a gyrotrope the current in the helix was reversed, and the effect of the reversion noted; permitting the current through the helix to flow in its last direction, by means of a second gyrotrope, the current which excited the cores was reversed; and finally, while the last magnetic polarity remained unaltered, the direction of the current in the helix was once more changed, and the consequent deportment of the bismuth bar was noted.

In making these experiments and exercising some judgment in the choice of the relation between the strength of the magnets and the strength of the current in the helix, the most complete mastery was attained over the motions of the bar. With one disposition of the forces the ends of the bar of bismuth were promptly repelled, with another arrangement they were just as promptly *attracted*. If, when moving towards the cores, the direction of the current in the helix was reversed, the motion of the bar was at once arrested, and attraction was converted into repulsion. If, while receding from the bar, the direction of the current in the helix was changed, the recession was stopped and the ends of the bar were attracted. The same results were obtained when, instead of changing the direction of the current in the helix, the polarity of the electro-magnetic cores was reversed. When the latter were so excited that the poles of the same quality were presented to the ends of the bismuth bar, the repulsion of the one pole was balanced by the attraction of the other, and under the influence of these opposing forces the bar stood still.

Pursuing the argument further, a south pole and a north pole were caused to act simultaneously upon *each* end of the bismuth bar; supposing one end of the latter to be repelled by a south pole, then, on the assumption of diamagnetic polarity, the same end would be attracted by a north pole; and permitting both poles to act upon it simultaneously from *opposite sides*, we may anticipate that the force tending to turn the bar will be greater than if only a single pole were used. To test this conclusion, *four* electro-magnetic cores were made use of; the two poles to the right of the bismuth bar were of the same name while the two to the left of the bismuth bar were of an opposite quality; with this arrangement the mechanical action upon the bar was greatly augmented, and the foregoing anticipation completely verified.

The bar employed in these experiments is unusually large, but it does not mark the practical limit of success. All the results obtained with this bar were obtained with another solid cylinder of bismuth 14 inches long and 1 inch in diameter. The corresponding experiments were made with bars of iron, and it was always found that the arrangement of forces which caused the attraction of the ends of the paramagnetic bar caused the repulsion of the ends of the diamagnetic one; while the disposition which caused the repulsion of the ends of the paramagnetic bar produced, in the most manifest manner, the *attraction* of the ends of the diamagnetic one. As regards the abstract question of polarity, the only difference between iron and bismuth is the comparatively intense action of the former. In the case of a magnetic body, whose capacity for magnetisation does not exceed that of bismuth, and in which coercive force is equally absent, no proof can be adduced in favor of the polarity of the former body that cannot be matched by proofs of equal value in every respect of the polarity of the latter.

The objections that have been and possibly may be used against diamagnetic polarity, are next considered, and some observations are made on the constitution of the magnetic field. The relation of our present knowledge of diamagnetic action to the theory of Weber, and to Ampère's hypothesis of molecular currents is stated; and in conclusion, the author dwells briefly upon those points of diamagnetic action wherein the views of M. Matteucci differ from his own.

3. *Further Experiments and Remarks on the Measurement of Heights by the Boiling Point of Water*; by Professor J. D. Forbes, (Edinb. N. Phil. J., [2], i, 174.)—This paper is in continuation of one printed in vol. xv. of the Edinburgh Royal Society's Transactions. The object of it is to test the correctnes of the method of observation and of calculating the results, then proposed, and to compare both with those of more recent authors, particularly of M. Regnault of Paris, and of Dr. Joseph D. Hooker.

The author finds the results of his subsequent observations in 1846 in the Alps, up to heights considerably above 10,000 feet, to agree well with those previously published, made in 1842. They combine in shewing a sensibly uniform fall of the boiling point at the rate of 1° for 543 feet of ascent (in a standard atmosphere at 32° of temperature), which differs only 6 feet (in defect) from his previous determination. The average deviation of the individual results from the formula is only $\frac{1}{12}$ of a degree (without regard to sign).

Barometer. Inches.	Boiling Point. Fahr.	Difference from my formula.	Difference from Regnault's formula.
20·77	194°·28	+ 0°·22	+ 0°·32
20·79	194·33	− 0·08	+ 0·01
22·40	197·94	− 0·04	+ 0·12
22·67	198·51	− 0·08	+ 0·06
23·15	199·52	− 0·07	+ 0·06
23·35	199·94	+ 0·01	+ 0·15
23·89	201·04	− 0·11	+ 0·03
23·99	201·24	− 0·09	+ 0.08
24·02	201·31	+ 0·04	− 0·20
24·105	201·47	− 0·17	+ 0·03
25·14	203·51	+ 0·04	+ 0·19
28·49	209·54	− 0·07	− 0.06

The agreement with M. Regnault's table is also extremely close; and considering the ordinary limits of error of such observations, the writer considers it nearly indifferent for elevations under 13,000 feet which method of calculation be used.

The consistency of the results shows that the method of observation (which differs in some respects from that commonly used) and the graduation of the thermometers were satisfactory.

On carefully examining Dr. Joseph Hooker's detailed results, (obligingly communicated by him,) which that naturalist considered to be incompatible with Professor Forbes's formula, it is shewn that the inconsistencies of observations are so considerable, that it is difficult to give a decided preference to one formula rather than another, for the purpose of representing them; but that up to heights of at least 13,000 feet, a *linear* formula, or one which assumes the lowering of the boiling point to be exactly proportional to the height, seems to express the observations as well as any other; and the rate of diminution is almost the same as that deduced from Professor Forbes's observations, or a lowering of 1° for 538 feet of ascent.

The author has little doubt that M. Regnault's table, (which was not published when he last wrote,) does really represent the law according to which water boils more accurately than the simpler linear formula, though the difference is in most cases insensible. For all ordinary heights (or up to 12,000 feet) Regnault's table may be more accurately represented by the formula, $h = 535\,T$;
where h is the height in English feet, T the lowering of the boiling point in Fahrenheit's degrees, reckoning from 212°. But he finds that Regnault's table may be represented in every case which can occur in practice, and with almost perfect accuracy, by the following formula, which is nearly as easy to use:—

$$h = 517\,T + T^2.$$

II. Mineralogy and Geology.

1. *Herrerite identical with Smithsonite*; by Dr. F. A. Genth, (Proc. Acad. Nat. Sci., vii, 232.)—When Herrera described an apple-green mineral from Albarradon in Mexico, having a distinct cleavage parallel with the planes of a rhombohedron, as a combination of 55·58 p. c. of tellurium, 12·32 p. c. of sesquioxyd of nickel and 31·86 of

carbonic acid, all mineralogists who were conversant with the laws of chemical combination, considered it as nothing more than a mechanical mixture, though the description appears to have been that of a simple substance. Afterwards Del Rio pronounced it "carbonate of zinc, with some nickel and cobalt," and this being a very probable composition, which also answers very well to the physical properties, it was generally considered a Smithsonite, though, (no other examination having been made of it,) as doubtful. I have received, through the kindness of Dr. J. L. LeConte, a genuine and perfectly pure piece of the mineral, which I have carefully examined. The physical properties of it agree very well with those given by Herrera. With regard to its chemical composition, Herrera has the merit of having correctly discovered in it the presence of carbonic acid; Del Rio, that of having found in it the ozyd of zinc. With regard to the other constituents, both were mistaken. A qualitative analysis has shown that this mineral contains carbonic acid, oxyd of zinc, oxyd of copper, oxyd of manganese, magnesia and lime, but no traces of any other substance. The quantitative analysis was made with the usual methods.

B.B. in a tube it blackens and does not give any "sublimate, which condenses in transparent drops," (as Herrera stated,) but none at all. On charcoal in the reducing flame it blackens, and covers it with white incrustations, having a steel-blue margin, which are yellow as long as hot; and which, when moistened with cobalt solution and re-heated, become green. The mineral itself does not become green, but brownish black; and after having received a good reducing heat, it shows under the magnifier, and when powdered in an agate mortar, metallic copper. With borax, it gives distinctly the copper reactions. Dissolves in acids slowly, with effervescence. After weighing, I have carefully examined all the separated substances for their purity, but neither traces of tellurium, nor those of cobalt and nickel, could be detected.

0·6226 grs. of the mineral gave:

0·3783 grs.	of oxyd of zinc, or - -	60·76 p. c	Zn O;
0.0137 "	" oxyd of copper, or - -	2·20 "	Cu O;
0·0062 "	" protosesquioxyd of manganese, or	0·93 "	Mn O;
0·0025 "	" pyrophosphate of magnesia, or	0·14 "	Mg O;
0·0092 "	" carbonate of lime, or - -	0·83 "	Ca O;

Herrerite is therefore nothing but a cupreous Smithsonite, and its composition the following:

Carbonate of zinc, - - - -	=	93·74 per cent.
" " copper, - - - -	=	3·42 "
" " manganese, - - - -	=	1·50 "
" " magnesia, - - - -	=	0·29 "
" " lime, - - - - -	=	1·48 "
		100·43

2. *Analyses of the Meteoric Iron from Tuczon, Province of Sonora, Mexico;* by Dr. F. A. Genth, (Proc. Acad. Nat. Sci., 1855, vii, 317.) —The masses of Meteoric Iron of Tuczon, first brought to notice by Dr. J. L. Le Conte, (Am. Journ. Sci, 2d Ser. xiii, 289,) and afterwards more fully described by Prof. C. U. Shepard, (Am. Journ. Sci., 2d Ser., xviii,

369,) are also mentioned by Dr. J. L. Smith in his Memoir on Meteorites, published in the last number of the same Journal, March, 1855, in which he gives the analysis of a piece cut from one of the huge masses by Lieut. Jno. G. Parke, of the U. S. Topographical Engineers. Several months ago I had finished the following analyses of the same meteorite, (which will be found to agree very well with those of Dr. Smith,) but various circumstances have heretofore prevented my presenting to the Academy the results of my examinations, which were made with pieces taken from the specimen presented to the collection of the Academy of Natural Sciences, by Dr. Heermann.

To the descriptions of this meteorite given by Prof. C. U. Shepard and Dr. J. L. Smith, I have to add only a few words.

The pieces which I have examined were not passive, as Prof. Shepard remarks, but dissolved readily in nitric acid, and also immediately precipitated metallic copper from the solution of the sulphate. On dissolving it in chlorohydric acid, only a slight odor of carburetted hydrogen was perceptible, and no gas evolved, which precipitated an ammoniacal solution of chlorid of copper; a very small quantity of Schreibersite separated in form of a brownish powder.

On evaporation of the solution by nitric acid in a water-bath and subsequent moistening of the dry mass by chlorohydric acid, all the substances were taken up, excepting a small residue of siliceous matter. This dissolved partly on boiling with carbonate of soda, leaving a residue, which I took for a feldspathic mineral; the quantity obtained, however, was too small for further examination; the small quantities of alkalies, lime and alumina speak in favor of this view, and indicate that the residue insoluble in carbonate of soda is labradorite, a mineral which is partly decomposed by acids. The silica, soluble in carbonate of soda, results undoubtedly from the decomposition of olivine. I was particularly careful to obtain the whole quantity of cobalt and nickel, and have therefore not separated the sesquioxyd of iron by carbonate of baryta, which method is not quite correct; but have precipitated the boiling solution, containing sesquioxyd of iron, oxyd of cobalt and nickel, magnesia, etc., and a sufficient quantity of chlorid of ammonium by ammonia, and immediately afterwards acidulated by acetic acid, boiled and filtered. All the other determinations were made in the usual way.

The following results were obtained:

	I.	II.	III.
Copper, -	0·008	not estimated.	not estimated.
Iron, . -	83·472	not estimated.	83·637
Cobalt, - -	0·420	0·366 }	9·851
Nickel, - -	9·441	8·689 }	
Chrome, -	not estimated.		0·174
Alumina, - -	traces.	traces.	traces.
Magnesia, - -	2·593	2·030	2·147
Lime, - -	0·463	0·550	not estimated.
Soda, -	not estimated.	not estimated.	0·174
Potash, -	not estimated.	not estimated.	0·098
Phosphorus, -	0·103	not estimated.	0·150
Silica, - -	2·889	not estimated. }	4·169
? Labradorite, -	1·046	not estimated. }	

3. *On the Physical Geology of the Himalaya;* by CAPTAIN RICHARD STRACHEY, F.R.S., F.G.S., (Jour. Geol. Soc., x, 249.)—The author having in a previous communication to the Society,* described the geological structure of a part of the Himalaya Mountains, in the present paper he proposed to complete that sketch, at the same time taking a rather larger field of view, offering some explanation of the manner in which the chain in general had been raised to its present position—first, with reference to the mechanical action of the forces in operation; and secondly, as to the epochs at which the upheavement took place.

Attention was first directed to some of the more prominent points of the physical structure of the great mountain mass, of which the Himalaya forms a portion. Among these were especially noticed:—1st. The general form of the section of the mass, which shows that the Himalaya mountains are merely the southern slope of a great protuberance, the summit of which forms the table-land of Tibet, while the northern slope is a mountainous region, marked on our maps as the Koneulun, similar to the Himalaya, and terminating in the great plains of Central Asia. 2nd. The parallelism to one another, and to the outer edge of the mountain area, of the great ridges or lines of elevation; of the great valleys or lines of drainage, which are also lines of rupture; of the strike of the strata; of the lines of igneous action; and of the distribution of the various deposits, considered both in reference to their mineral character and geological age. 3d. The arrangements of the drainage, in accordance with which the crests of the northern and southern slopes of the great mass form two main lines of water-shed, proposed to be termed the "Turkish and Indian Water-sheds," to the north and south of which, respectively, the rivers flow off directly to the plains of Turkistan on one side, and to those of Northern India on the other; while the waters of the summit of the table-land are collected into two streams alone, the Brahmaputra and the Indus, which are discharged from the mountains at two distant points, at opposite ends of the chain. 4th. The constancy maintained for great distances along the length of the chain, both in the geological structure, and in the elevations to which the mountains rise; which last character is particularly shown by the almost perfect horizontality of some of the most elevated stratified deposits, by the extremely small slope of the main drainage-channels of the Tibetan table-land, and by the general similarity of the altitudes even along the most rugged ridges.

After allusion to the question of the reality of upheavements, as opposed to the view which attributes elevations on the earth's surface to subsidences of the lower parts, and having stated reasons for believing that in the present instance the elevation was real, a review was taken of the igneous rocks which might appear to have been connected with the upheavements of these mountains, and the conclusion was arrived at, that none of them were specially related to the last great movement that had taken place.

Hence it was inferred that the agent of elevation was probably a development of elastic vapors, as has been considered by Mr. Hopkins to be likely in a general point of view. The probability of this in the

* Quart. Journ. Geol. Soc., vol. vii, p. 292

present instance was shown to be upheld by the the extreme regularity of the upheavement, when regarded as a whole, which seems to indicate a fluid body as the medium through which the forces have been applied to the superincumbent masses; while this regularity also shows the comparatively great thickness of the part of the earth's crust that has been raised, as compared to the heights of individual ridges or peaks above the general level.

Assuming the general unity of the upheavement of the whole area as evident from the consideration of the form of the section of the mountains, the results of such an upheavement were then examined by help of the principles laid down by Mr. Hopkins.

The first of these results was stated to be a main system of longitudinal fissures, parallel to the major axis of the elevated area; with a secondary system perpendicular to the former. As the manner in which these fissures would open, and all the subsequent effects of the upheavement, must depend in great measure on the state of the interior of the earth, of which we can know nothing directly, we must be guided in our suppositions on this point by the phenomena actually observed in connexion with the ruptures at the surface; and, in the present case, we seem to be able to account for many of the peculiarities of structure already noted, by conceiving that the state of the upheaved mass, at the moment of rupture, was similar to, but just the reverse of, that of a beam supported and held down at the ends, and loaded at the centre.

On this hypothesis we should expect to find the chief longitudinal ruptures in the centre of the area, with others important, but in a less degree, along the margins, while there would be two portions intermediate between the axis and the edges, where the tendency to rupture would be a minimum. Further, as the tendency to transverse rupture is proportional to the amount of the elevation, it would have a minimum along the edges of the area; at the same time, as in any part of the mass, the tendency to transverse rupture would be least where the general coherency was least disturbed, we should expect to find fewest transverse fissures on those lines of minimum longitudinal fracture just noticed.

Comparing these theoretical views with the observed facts, we find:—

1st. The existence in a marked degree, of the longitudinal and transverse fissures, in every part of the mountains.

2nd. The more open character and greater importance of the longitudinal fissures in the centre of the area, as evinced by the direction of rivers on the Tibetan table-land.

3d. The existence of an important line of fissure along the outer margin of the Himalayan slope, as proved by the narrow fringe of the latest formations that everywhere skirts the foot of the mountains, which it is impossible to suppose could have been raised as they are, excepting in connexion with some larger mass.

4th. The occurence of two lines of least rupture running parallel to the margin of the area, and intermediate between it and the axis, indicated by the Indian and Turkish water-sheds; these features being dependent for their existence, not on any superior elevation that they attain over other parts of the chain, but only on their continuity, and on the few transverse fissures by which they are broken through.

5th. The few transverse fissures along the outer margin of the area, which affords a probable explanation of the accumulation of the drainage of considerable areas of the Himalayan slope, into the few great rivers that pass through the outer ranges.

After entering into some details as to the more important lines of fissure, it was remarked that the presence of more fissures than may seem to be necessary, or of others not in accordance with these views, may be readily expected to have been caused in movements anterior to the last great elevation. At the same time, the general parallelism of all the anterior movements to the last is indicated by the constant parallelism of the strike of the strata, the lines of rupture, the lines of elevation, and those of the directions of the stratified deposits; although a certain obliquity at the same time does exist, which, however, must be more closely examined before more can be said of it.

The author then passed to the chronological part of the subject. The age of the granite of the great line of snowy peaks was first considered, and shown to be probably anterior to the Silurian period; but doubts still remain on this subject, though the occurrence of rolled pebbles of this rock in the tertiary deposits of the Tibetan table-land, show that the eruption was certainly antecedent to the middle of the tertiary epoch.

The existence of a coast-line along the Indian water-shed, as indicated by a conglomerate-bed at the bottom of the Silurian rocks, was next noticed, with the presumption of the existence of a northern sea, having its boundary along this line during the Silurian epoch, as well as during the Jurassic, and the earlier portion of the Tertiary period. On the Southern edge of the mountains there is nothing to prove the boundary of the land and sea, until the Tertiary period, when the coast-line is found along the general line of the existing outer hills. Still, the existence of fossiliferous beds in the salt-range in the north of the Punjab, apparently synchronous with those of the Indian water-shed, seems to render it probable that there was a Southern sea, contemporaneous with the Northern, extending over the existing planes of North India, from the remotest times, and leaving an area of dry land between them, on what is now the Himalayan slope. The probability of this area of dry land is further shown by the almost total absence of fossils in these regions; nor does the existence of fossils at one single point in Kashmir, where only they have been found, indicate more than an irregularity of the outline of the land, such as might have been anticipated.

Turning next to the question of the dip, which, as a general rule is everywhere towards the axis of the elevation, it was stated that there appeared to be nothing to connect this inclination with the last elevation, and that it is more probably due to some of the earlier movements of the surface. It was shown how an inclination, having been once imparted to any dislocated mass of the earth's crust, any subsequent motion would most likely maintain it, or even add to its amount; for the tilting of a mass displaces the position of the centre of gravity, so that it is no longer immediately over the centre of the base of the mass, at which the resultant of all elevating forces will act; and hence, in any subsequent elevation the weight of the mass will tend to make it re-

volve, by the relative descent of the centre of gravity, and ascent of the point of application of the elevating forces, which will evidently tend to increase the dip.

The dip of the outer hills, however, which is almost everywhere towards the interior of the chain, just as is the case with the rest of the mountains, cannot be attributed to these older movements, for these ranges are among the most recent of the whole; nor is there, at first sight, any very evident connexion between the dip that they might be supposed to assume, and that of the older ranges beyond them. The phenomenon may, however, perhaps be explained, by supposing that the fissure which we have reason to believe to exist along the foot of the mountains was originally covered up by the deposits which now form the outer hills, when the ocean extended over the plains of Northern India. An upheavement of the mountains alone, the general sea-bottom remaining unmoved, would naturally terminate at the fissure, and a narrow fringe of the younger beds would be raised on the flank of the older mass, to which a dip the same as that of the older mass might be imparted, by that tendency to revolve already explained. The repetition of this process would make a succession of ranges, such as are seen in the outer hills, apparently dipping under one another and the older beds, of the fragments of which they are evidently made up, in an inverse order.

The paper concluded by a recapitulation of the progress of the Himalayan chain, as far as it could be traced, from the earliest geological period to the present time. In this was pointed out the probable existence of land with mountains already of considerable altitude, in the earliest ages; and to the general elevation of these older heights with the rest of the area, the position of the great line of Himalayan peaks was attributed, rather than to any especial energy of upheaval along that particular line in any of the later changes. Until the commencement of the Tertiary period we can say little of the state of the area, but then the nummulitic sea may be traced extending across the greater part of Southern Asia, through Asia Minor, Persia, and Afghanistan, and along the foot of the Himalaya, even to the Kahasiya hills, east of Bengal. The mountain-area, above the level of the sea was at this time probably restricted, for the traces of the ocean of that period have been found in Tibet, at a place where a pass now rises to a height of 16,500 feet. At the close of the nummulitic period, a general elevation seems to have taken place over all Southern Asia, and the table-land of Afghanistan was finally raised above the Southern sea, which, however, still swept over the whole of the existing plains of India; while the Caspian probably extended over all the steppes of Western Turkistan, to the foot of the Hindu-kùsh. To the north of the Himalayas, the Northern sea appears to have covered the table-land of Tibet, occupying long fiords or estuaries between the mountain-ranges, which had already commenced to rise.

By the middle of the Tertiary epoch the Siwalik Range had been brought into its existing position, the whole of the great Tibeto-himalayan system had been greatly raised, and the upward motion extended to the basin of the Caspian, which sea was now separated from that of Aral. But the Indian Ocean probably still covered the northern plains

of India, nor was it until long after that it retired to its existing shores. Finally, the evidences of the diminution of the Himalayan glaciers in the existing epoch, which are to be met with at all parts of the chain, were attributed to the change of climate that must have accompanied this last change of the marine area, by which the conditions, becoming more *continental*, the summer temperatures would be raised, and the fall of snow diminished. This, therefore, might have had the effect of causing the snow-line and glaciers to recede, although the actual elevation of the mountains had been increased about 1000 feet.

4. *Eruption of Vesuvius*, (from letters in London Daily News, dated Naples, May 5 and 10, 1855.)—*Saturday*, *May* 5.—Having purchased our torches at Resina, we turned out of the high road into the comparatively narrow and heavy route which begins the ascent. It is formed of loose volcanic dust and pulverized lava; and hard work it is indeed for the weary horses to get along. Ours acted most prudently by refusing to advance, so that, dismounting, we took to our legs. A woman might have gone up alone, so dense were the crowds either coming or going; for be it known that apart from curiosity many felt not a little relief at the eruption, as though it had saved them from the disasters of an earthquake, and were full therefore of joyousness. As we got close under the mountain we experienced something like disappointment, for the elevation on which the Hermitage stands hid from our view the fire and smoke and the streams of lava which even from Naples formed so magnificent a spectacle. As we got higher and higher the glare of light reflected on the sky became visible, and by the time we had got to the Hermitage the grandeur of the scene began to open upon us. Pushing on from this point through cicerones and donkeys, horses, carriages and christians, as bipeds are styled in Naples, we traversed for some distance the road which leads to the cone, when, turning sharp off to the left, we arrived at a point on the stream of lava where it forms a cascade. This is about a mile or a mile and a half from its source; and here a vast crowd was assembled, as though it afforded the grandest *colpo d'occhio*. To tell the truth, your correspondent labors to write coolly, for such was the magnificence of the scene that were one to trust too much to his feelings he would be hurried into what might appear exaggeration. The lava on which we stood was yesterday a boiling, moving stream; it was still hot to our feet, and taking up the loose pieces of scoria the glowing fire was clearly perceptible beneath; we lit our cigars at it, and played all kinds of practical jokes; and, jumping over the fiery fissures, stood by the side of the bed of moving lava. Imagine thousands of tuns of coke carted out together, and rolling mass over mass, and some idea may be formed of the noise—not that it was loud, but a wide spread incessant sh–sh–sh–sh–like water over pebbles. When a great accumulation of materials had been formed at the edge of the precipice, the outer blackened crust broke up, and rolling over into the abyss below, bounded from rock to rock down among the chestnut trees, which writhed and flamed up, and then fell over. The immense body of heat and light which then burst forth nearly scorched and blinded us, and instinctively we held up our hands to ward it off. The breadth of the stream in this direction is 100 palms, says the Neapolitan journal; from my observation, I should say nearer 200 palms.

Of course all calculation must be mere guess-work, as who can measure a fiery flood? I never witnessed such mighty results of power apparently so unconnected with any cause. It produced the same impression upon me that the Toledo or Strand might do were either to take it into its head to walk. There was a solid plain which we might have crossed some eight and forty hours before now going full drive over a precipice some thirty or forty feet deep, and then stealing onward, as it now is, through chestnut groves and vineyards and villages, and threatening places of some consideration. Above the precipice the stream—or rather two streams, which are united at the cataract—flows through a plain in a serpentine form, and following back its course we arrived at the foot of the cone. Half way up, we came upon the first of seven mouths, all of which threw out either lava or stones, or both. Those which threw out lava bubbled and gurgled over, while those which made a greater effort and threw out stones, kept up an incessant noise, as that of a distant, heavy cannonade. What a foreground was this! Behind these in the distance the background was formed of heavy masses of lurid clouds, showing off by a strong contrast the vivid fires in front. The lava flowed down the sides of the mountain in waves of fire, and rolling through the valley we had just passed, precipitated itself into the gulf above which we stood at first. Of course we ladled up some liquid lava and fixed some coppers in it, and then were glad to move off. The wind occasionally shifted, and with it that curtain of lurid clouds. Our guide warned us to be off on penalty of sharing the fate of Pliny, a consummation earnestly to be avoided, we thought, however great our respect for the old Roman. Since that evening the seven mouths have all resolved themselves into one. The lava still continues to pour down the mountain in the direction of San Sebastiano, Pollena and Massa di Somme, being the same direction it took in 1822. It has already occupied the bed of a river, destroyed the plantations, and is threatening still greater injury. The Marchesa St. Angelo, who has property in that direction, has been removing his furniture from his villa as a precautionary measure. In short, this eruption, though not so effective a spectacle at a distance as the last, is far more imposing when viewed close, and threatens to be much more destructive.

Thursday, May 10.—The lava has now advanced ten miles from its source and is doing terrible damage. I have before me the report of Cozzolino as to the latest changes which have taken place about the cone. Just at the base of it a lake of fire has been formed which looks like a red sea in an undulatory state. In the very center of this has opened another crater, which is throwing out red-hot stones. On the morning of the 7th the crater at the very summit fired, as it were, two heavy cannonades; and after sending forth lightning, flames and stones, broke up altogether. In the middle of the cone ten craters have been formed, and from these the lava pours forth like a river and runs on the side of the Cavallo as far as the Minatore. Here four other craters have been formed, which resemble gigantic exhibitions of fireworks. The whole of the summit is therefore like a sponge and must inevitably fall in. The thin crust trembles under your feet. You may see the stones dance with the tremulous movement; the part immediately round the crater

looks like the sides of a heated copper boiler. Such is a true statement of what is going on at the summit. There are reports of an opening toward Pompeii, which is not unlikely, and of another toward Resina, but I have not been up for some days, as the danger is now very great. Before I write again I shall make the attempt.—Last night I went to the scene of most stirring interest after an interval of two days. The whole length of this usually quiet road was like a fair, and such was the throng of carriages which were moving on in three lines that it was with difficulty we ever arrived at our destination. As we approached the menaced neighborhood the inhabitants were removing their goods, and on a bridge in the middle of the little township of Cercolo (through which in the winter time thunders down from the summit of Vesuvius one of those mountain rivers so well known in Italy) stood a company of sappers. Creeping under the solid handsome bridge into the bed of the river, we went up in face of the lava, which, was now coming rapidly down. Here again were sappers, raising mounds on either side to divert the ruin from some private grounds and keep the lava in one straight course. The smoke which rose over the heads of the multitudes told us we were close on the spot, and climbing up the bank and walking along the top we looked down on this mighty mass of fire. How changed the neighborhood in two days? Where I walked on Sunday night was now a sea of fire. The side road by which I had come down into the main stream from Pollena and Massa di Somme was now full of blackened coke. The houses on the borders of the village had fallen—in one thirty poor people lived; a small chapel was swallowed up, a gentleman's villa, and a sad extent of vineyard and garden ground. On the other side of the great lava bed another stream was branching of to San Sebastiano. We had hoped to cross it and ascended to the cascade again, but it was no longer possible; for as one says speaking of a marshy country in the winter, the lava was out. The fire here had begun to enter the burial-ground of the little town, but was diverted from its course by a wall. On the opposite side of the stream were the king and all the royal family. The banks on either side were thronged with curious and anxious multitudes, whose faces were lighted up with the blaze of hundreds of torches and with the more resplendent light of the rapidly-descending lava. Since the morning it had moved a mile. It was like a vast river of glowing coke. As it moved on, the tens of thousands of lumps rolled and tumbled one over the other, crackling and grinding and grating; and when from the very face of it a large lump fell off, the appearance was that of an iron furnace when the iron is being drawn. To make the resemblance more complete, at such times men darted forward with long poles taken from the neighboring vineyards, and pulled out great masses of lava in which they imbedded money for sale. What struck me at first, and still strikes me as the most majestic feature in the whole scene, is the slow, silent, irresistible motion of that fiery flood. Active almighty power without an effort! Sweeping everything before it, overcoming every obstacle, growing up against intervening walls or houses, and devouring them bodily, and then marching on in the same silent, unrelenting, irresistible manner as before. There was a spot beneath my feet where a fall of mason work had been built to break the violence of the winter

floods: to this spot all eyes were directed. The fiery river would fall over it in an hour; as yet it was distant from it seventy yards perhaps. Gradually it rose in height and swelled out its vast proportions, and then vast masses fell off and rolled forward; then it swelled again as fresh matter came pressing down behind, and so it broke, and on it rolled again and again till it had arrived at the very edge. There was a general buzz and murmur of voices. The royal family stood opposite to me intermingled with the crowd looking on with intense anxiety. At last it broke, not hurriedly, still with a certain show of majesty. At first a few small lumps fell down; then poured over a pure liquid of metal like thick treacle, clinging sometimes mass to mass, from its glutinous character, and last of all tumbled over gigantic lumps of scoria. Then on it moved once more in its silent regular course, swelling up and spreading over the vineyards on either side; and now there was a rush for the road which traverses this lava-bed. Houses and the bridge bordered the road, the carriages had all been ordered off, and the bridge was being broken down—we were cut off completely. The sentinels would not let us pass, and struck us and drove us back; but we forced our way, and then found too surely that it was impossible to get on. The bridge was half demolished, and by the light of the torches we could see the soldiers above working away with the pick and the axe. We had therefore to retrace our steps, and making a long circuit through the open country and over walls, came round to the top of the bridge. "Run" said the sentinels, "or you will be too late." We crossed the narrow parapet which was still remaining, and soon afterward down went the whole fabric. In this way it is hoped that the lava will be diverted from the townships of St. Sebastiano, Massa di Somme and Pollena, which stand on either side and have as yet only suffered partially. Cercolo, through which, however, the stream is rolling, will be sacrificed. The expectation is that the lava, should the eruption continue, will flow down to the Ponte Maddaloni and into the sea. So grand and so destructive an eruption has not been known for many years, and even now we cannot tell how or when it will terminate. The mountain is literally seamed with lava and many fear a violent explosion as the final scene of the tragedy.

5. *A Manual of Elementary Geology*, or The Ancient Changes of the Earth and its Inhabitants as illustrated by geological records; by Sir Charles Lyell, M.A., F.R.S., &c. 5th edit. greatly enlarged, and illustrated with 750 woodcuts. Boston, Little, Brown & Co. 1855.—This new edition of "Lyell's Elementary Geology" has received very large alterations and additions, much new matter being introduced, both from the extended investigations of the author, and the various researches of other geologists. It is the ablest and fullest work of the kind in the English language.

The edition issued by Messrs. Little, Brown & Co., is the English edition, and not a reprint, having therefore all the perfections of the original work in the cuts and letter press.

6. *Fossils of South Carolina;* by Prof. M. Tuomey and Prof. F. S. Holmes, 4to. Charleston, S. C.—No. 2 of this elegant work has just been issued.

7. *Naturgeschichte der Vulcane und der damit in Verbindung stehenden Erscheinungen*, von Dr. GEORGE LANDGREBE; Mitgliede mehrerer gelehrten Gesellschaften. 2 vols., 8vo. Gotha, 1855.—Dr. Landgrebe has given in this work condensed accounts of all the volcanoes of the world in the first volume, and general views on their distribution, the nature of volcanic rocks and other topics in the second. He has overlooked the recent accounts of the Hawaian volcanoes in this Journal, and also the Geological Report by J. D. Dana, in which a large part is devoted to the subject of volcanic islands and phenomena.

8. *Lehrbuch der chemischen und physikalischen Geologie*, von Dr. GUSTAV BISCHOF. Bonn. Part 7 of this excellent work on Chemical Geology, has recently been issued.

9. *Uebersicht der Resultate mineralogischer Forschungen* im Jahre 1853, von Dr. ADOLF KENNGOTT. Leipzig, 1855. T. O. Weigel.—This is another issue of Dr. Kenngott's Mineralogical Annual, containing abstracts of all papers published on Mineralogy and related topics during the year 1853. It extends to 174 pages quarto. We shall return to it again in a future number of this Journal.

10. *Versuch einer Monographie des Borazites*; Eine fassliche angewandte Darstellung des jetzigen Standes der Krystallologie und ihrer neuesten Richtung: Ein Beitrag zur Geschichte dieser Wissenchaft und zur Kenntniss der Steinsalz-Lagerstätten und ihrer Bildung; von G. H. OTTO VOLGER, 244 pp., 8vo. Hannover, 1855, C. Rümpler.

11. *Materialen zur Mineralogie Russlands*, von NIKOLAI VON KOKSCHAROW, Berg-Ingenieur, Mitglied der R. K. Mineralogischen Geselschaft zu St. Petersburg, und des Naturforschenden Vereins zu Moskau, Lief. 6, 7, 8, 9, 10, 11, 12. 8vo, with plates in 4to. St. Petersburg, 1854.—M. Kokscharov, in these numbers continues his excellent work on Russian Mineralogy. A large number of new forms of crystals are carefully figured in the 4to plates. With part 9 the second volume of the work commences. We shall make citations from the work at another time.

12. *Die geologische Uebersichtskarte des mittleren Theiles von Süd-Amerika*, von FRANZ FOETTERLE; mit einem Vorworte von W. Haidinger. 22 pp., 8vo. Wien, 1854.—The geological map of South America accompanying this pamphlet is nearly two feet square. It is colored to represent the geological formations, and combines the results of the observations of various travellers. The details are more fully and exactly given with respect to Brazil, as stated in its title.

III. BOTANY AND ZOOLOGY.

1. "*Poetry of the Vegetable World: a Popular Exposition of the Science of Botany and its relations to Man*; by M. J. SCHLEIDEN, M.D., Professor of Botany in the University of Jena. Illustrated with engravings. First American, from the London Edition of Henfrey. Edited by ALPHONSO WOOD, M.A." Cincinnati and New York, 1853.—The first and principal remark we are called to make upon this book is that it appears under a false title. Prof. Schleiden, at the solicitation of friends, committed to the press, in 1847, a discursive course of popular lectures upon the *Plant and its Life*. His interesting and very

characteristic volume, entitled "*Die Pflanze und ihr Leben; Populäre Vorträge*, was admirably translated in England by Mr. Henfrey, and published by Bailliere in 1848, under the English title of "The Plant; a Biography; in a series of Popular Lectures." The English publisher has beautifully reproduced the plates and the vignettes; and Mr. Henfrey's known ability is a guarantee that the text is faithfully rendered. In causing the book to be reprinted in this country, where it had already become pretty well known, Mr. Wood has taken what we must call the unwarrantable liberty of altering the title, as above.

What was the object of the change is more than we can imagine. Perhaps the factitious title was thought to be a more taking one than the real. So, indeed, it lately proved in the case of a friend of ours, who was *taken in* by it. Having purchased the new book by its title, he found, to his chagrin, that it was only a reprint (with the pretty vignettes and engraved frontispiece left out) of "The Plant," translated by Henfrey, a copy of which had long adorned his library. This mishap makes us regret that we had neglected to indicate this danger when the American reprint made its appearance in such a guise. We fully absolve Professor Wood from any equivocal intention in the matter; but taking liberties with names and title-pages is neither proper nor safe. The omission of the vignettes at the head of each lecture and of the engraved frontispiece might have been excused if the fact had been mentioned. The paragraph which relates to them in the author's introduction is duly given, as is a reference or two in the body of the work; but the pictures themselves have silently vanished; and some of the notes referring to them are mutilated so that the innocent reader would not detect the omission. So also Mr. Henfrey's name has almost vanished from the title page of his own literary production. We likewise miss Henfrey's sensible advertisement, stating that he had confined himself to the simple rendering of Schleiden's language into English, retaining "the peculiar character of the original," and leaving its faults and its beauties,—equally characteristic of the able but excentric author—to speak for themselves. The author himself, in his introduction, takes special pains to place this volume before the public in the proper light, namely, as a course of popular lectures, cast in a mould befitting for oral delivery, but not to be judged by the rules applicable to a treatise or a series of essays. Nevertheless, for some unaccountable reason, the American editor has thought fit to change this point of view as much as possible, by sedulously substituting the word '*Essays*' for '*Lectures*' throughout the Introduction, and by transforming the twelve 'Lectures' into 'Chapters:' while the word 'lecture' is carefully weeded out wherever it occurred in the discourses, and chapter awkwardly substituted for it. Turning to his preface, we find no explanation of this interference beyond the remark that: "In bringing this volume before the American public, we have *availed ourselves* of the English translation of Arthur Henfrey, F.L.S. The only objections to this excellent version which have appeared obvious to us were such as resulted from the translator's fidelity. In endeavoring to preserve, by a literal rendering, the true spirit and the glowing style of the original, he has given us occasionally sentences too long and involved, and sometimes too much encumbered with epithets to suit the genius of our language. In such

cases, we have frequently ventured to substitute a more simple version of our own." If what we have noticed are specimens of the substitutions referred to, we can only say that the objection of *fidelity* to the language and *true spirit* of the original is removed by the American editor as far as possible under the circumstances; but the *simplicity* of the new version is more apparent to us than its propriety or taste. A. G.

2. *De Vriese and Harting: Monographie des Marattiacées.* Leyden and Dusseldorf. 1853. Elephant 4to, pp. 60, with 9 plates.—The systematic part of this work is by Prof. De Vriese of Leyden. The researches upon the anatomy and organogeny of the genus *Angiopteris*, followed by considerations upon the structure of Ferns in general, are by Prof. Harting of Utrecht. Both parts possess much interest for botanists. The number of species appears to be unduly multiplied. To one of de Vriese's new species from Luzon must belong Mr. Brackenridge's *Angiopteris attenuata*, of the Ferns of the U. S. Exploring Expedition,—a volume long since printed, but the publication of which is unaccountably delayed. A. G.

3. *Pritzel: Iconum Botanicarum Index Locupletissimus.* Berlin. 1854. pp. 1183, imp. 8vo.—A most useful volume, containing references to 80,000 botanical figures; the names arranged in alphabetical order, in double colums, closely printed, in very neat style. The price of the book is very moderate. A. G.

4. *Botany of the Southern States.* In two parts. I. Structural and Physiological Botany and Vegetable Products. II. Descriptions of Southern Plants, arranged on the Natural System, preceded by a Linnæan and Dichotomous Analysis. By Prof. JOHN DARBY, A.M. New York: Barnes & Co. 1855. pp. 612, 12mo.—Prof. Darby is a distinguished teacher, and he has devoted much pains and study to the preparation of the present work, especially to the structural and physiological part of it. In this department he has been an original observer; while in Systematic Botany,—which has many more votaries in this country, pursuing it to a certain extent,—he would probably claim to have a good general, but no very profound acquaintance. The book is excellently planned, and is well adapted for its purpose,—that of a text-book for the Colleges and High Schools of the Southern States. While we cordially commend it in this view, we are free to say that a cursory perusal has indicated not a few points which are open to criticism. Most of them, at least in the structural part, however, are cases in which views once maintained by writers of authority, or even still current, are adopted long after they have ceased to be tenable. The remark may also apply to some original suggestions, such as that of the use or final cause of spiral vessels in the vegetable economy, and that on the structure of the monocotyledonous stem as compared with the dicotyledonous. An inspection of any herbaceous stems of these two classes of plants suffices to show that they belong to two distinct types, which cannot be reduced to one in the manner that Professor Darby supposes, whatever view be taken on the vexed question of endogenous growth. It is now well understood that the name of Endogen is not a very happy one; yet the structure of such stems is none the less distinct from the exogenous structure. On Prof. Darby's theory, if we

rightly understand it, the stem of a *Cycas* or a *Cereus* should exhibit the endogenous, and a corn-stalk or asparagus-shoot the exogenous, structure. But this is not the place to discuss the question.

The vitality of an erroneous statement is truly wonderful. A century ago Adanson published his account of the great Baobab trees of Senegal and Cape Verde, which account was corrupted by one or more popular writers about thirty years ago, in such a way as totally to mislead authors of high authority, such as the De Candolle's, and to give general currency to an estimate of the longevity of these trees which was totally unwarranted by the facts as narrated by Adanson. This falsification was pointed out eleven years ago, by the present writer (in the North American Review for July 1844): nevertheless the current statement has been reiterated ever since, in various works, of considerable authority or pretension. Prof. Darby has now repeated it with an addition which caps the climax of absurdity. He says,—doubtless copying from some other work, we know not what one:—

"A remarkable case of the deposition of external layers of dicotyledonous stems is related of the Baobab-tree (*Adansonia digitata*) of the Cape de Verde Islands. In the year 1400 Grew cut his name on two of these trees, and in 1749 (349 years afterwards) Adanson examined the same trees, and found the names with more than 300 layers of wood deposited over them." (p. 31.)

We had long ago shown that Adanson never pretended to have found any such thing, and that the current account asserting this was founded upon some strange mistatement or pure fiction. But in the present narration, *Grew*, the Vegetable Anatomist, who flourished in the latter part of the seventeenth century, and who perhaps was never out of England in his life, is said to have cut his name upon two Baobab-trees at the Cape de Verde Islands in the year 1400!

The old notion of *spongioles* at the extremities of roots,—organs which have no existence as described—is adopted on p. 39: but in the description they are confounded with the root-hairs, which are not the *spongioles* of De Candolle, nor are they "composed of lax cellular tissue," being only definite and free portions of superficial cells prolonged into tubes.

So far from the origin of adventitious buds having "completely eluded the research of philosophers," no part of organogeny is better known. The facts of the case are to be found, however, not in the speculations of Duhamel and Knight, or the assumption of Lindley (that their origin is connected with an anastomosis of woody fibre), but in the neat microscopical researches of Trécul and other modern observers.

The statement that the leaves of Pines, &c., "do not form a single spire, but a compound one, consisting of three or four spires running parallel to each other," shows that the writer has not discriminated between the real spiral on which the leaves are arranged, and the apparent or secondary spirals which necessarily become most conspicuous when the leaves are approximated. A moment's attention to the subject would enable Prof. Darby to express the elementary facts of *phyllotaxis* both correctly and clearly.

The fall of the leaf by an articulation has surely been much better explained by investigations of the facts of the case, by no means of

very recent date, than by Lindley's crude and imperfect hypothesis, which is most satisfactory to the author. For the want of a notice of *errata* (which are numerous, owing to the printing of the work at a great distance from the author's residence) we cannot ascertain what species is intended, on p. 60, by *Magnolia heterophylla.*

But what surprises us most is to find, now and then, in the course of a chapter which is on the whole clear and satisfactory, some gross absurdity like the following:

"Raspail asserts that the pollen is a production of the internal surface of cells within the theca, to which the grains are attached by a funicle. This is denied by other botanists." (p. 78.)

This is all that is given on the subject of the formation of pollen,—a subject on which perhaps it was not necessary to say anything in so elementary a treatise; but whatever is stated should have some resemblance to or compatibility with what is true, or what is or has been generally thought to be true. Whereas this statement is worse than that of the writer of an introduction to Astronomy would be who informed his pupils that: Ptolemy asserted that the sun and the other heavenly luminaries revolve around the earth: but this is denied by other astronomers:—inasmuch as Ptolemy was an authority in his day, and his view was long received, while Raspail's never was, the entire absurdity of it having been made as patent before Raspail wrote as it is now; and the formation of pollen is fully explained in most introductory works.

The crude, but long current notion that a young stem bends towards the light because "the side next the light deposits its carbon, and becomes firmer, and is contracted, while the other side remains turgid with unassimilated fluid," it is not so surprising that our author should continue to adopt. Yet, as he has evidently given particular attention to physiological questions, he ought to have known, before the year 1855, that this hypothesis has been demonstratively proved to have no foundation, as he might have learned from the pages of this Journal, and from editions of the principal text-books now two or three years old.

In respect to so large and so common an embryo as the Horse-chestnut, it is curious to read: "there is apparently but one cotyledon. Prof. Lindley says, that, by dissection, there is a slit which indicates the division between the two bases of a pair of opposite confluent cotyledons." It is hardly worth while to bring in the dictum of a distinguished foreign professor to vouch for a fact which the youngest of Prof. Darby's pupils could verify in a moment, by slicing a horse-chestnut, or observe directly when these large seeds germinate in the spring. If, however, these pupils proceed to verify, upon a grain of corn, or the seed of most other common Endogens, the Professor's definition of the monocotyledonous embryo; viz, that "it is a homogeneous, cylindrical body, tapering at both ends, with no distinction of radicle, plumula or cotyledon," they will naturally be not a little puzzled by the total discrepancy between the definition and the fact.

Our author gives an account of the formation of the embryo on the Schleidenian hypothesis alone, thus impliedly by adopting that view; but without making the least allusion to the mass of observations, of which he cannot be wholly ignorant, which have negatived every es-

sential point of the hypothesis, and every presumption that may have existed in its favor.

The only additional remark for which we have room relates to a question of taste. We are confident that our excellent friend, on being made aware of the idea which his paragraph, No. 241, conveys solely through the unfortunate choice of a phrase, will in another edition no longer speak of our Savior as *yielding His assent* to the sentiment of a quotation from Young's Night Thoughts. A. G.

5. *Wheat from Ægilops.*—The announcement by Prof. Dunal, of Montpellier, two or three years ago, that M. Fabre, of that vicinity, had converted *Ægilops triticoides* into wheat, by cultivation for several generations, excited a lively sensation, which has not yet subsided. Prof. Dunal appears to have satisfied himself that *Ægilops triticoides*, or rather its ancestor, *Æ. ovata*,—a common grass on the southern coast of France,—is the original of wheat, and Prof. Lindley has adopted this opinion. This, if true, would be the only instance in which any one of the staple cereal grains has been identified with a wild original. Dunal's published account has called forth many detailed discussions, in which various views have been maintained: but at length a few simple experiments of M. Godron of Besançon appear to have disposed of the question. M. Godron's paper is published in the *Annales des Sciences Naturelles*, vol. ii. for 1854, No. 4. He remarks, in the first place, that the so-called species, *Ægilops triticoides*, is only of sparse and occasional occurrence; that it is seldom if ever found except in the vicinity of wheat fields, and in the districts where *Æ. ovata* abounds as a wild plant; that intermediate states between *Æ. ovata* and *Æ. triticoides* do not occur, as they are apt to do between races or varieties of any species; but that *Æ. triticoides* itself varies in certain respects and according to the kind of wheat which is cultivated in the neighborhood; and, finally, that the wild *Æ. triticoides* usually produces very little seed. From these considerations he was naturally led to suspect *Æ. triticoides* to be a hybrid, resulting from the accidental fecundation of *Æ. ovata* by the pollen of wheat. And this conclusion he has verified by direct experiment; that is, he has raised *Æ. triticoides* from seeds produced by impregnating the ovaries of *Æ. ovata* by wheat pollen. At the same time, and in the same manner, M. Godron produced a new and analogous hybrid by impregnating *Ægilops triaristata* with the pollen of common wheat; as well as another by impregnating *Æ. ovata* with the pollen of *Bearded Spelt* (*Triticum Spelta, barbatum*). It seems, therefore, most probable that M. Fabre's Ægilops-wheat owed its origin to the accidental fertilization of the *Æ. triticoides* experimented with—itself a hybrid between wheat and *Æ. ovata*—by the pollen of its male parent, wafted from adjacent wheat-fields; the cross-breed returning to the male type in the usual manner under such circumstances.

This evidence, however, does not convince Dr. Lindley; who objects that M. Godron and others have not explained what the origin of wheat has been, if it is not a domesticated condition of *Ægilops*:—an undertaking which we must say is by no means incumbent upon M. Godron, who had accomplished his object when he has shown, as he has clearly done, that M. Fabre's famous experiments do not prove Ægilops to be

the original of wheat;—although in his opinion the two should be ranked in the same genus. Can Dr. Lindley indicate the wild original of Maize, Rye, Barley, or Rice? And does the fact that the originals (if they indeed exist) are unknown, render it any more likely that these cereal grains are the progeny, in altered guise, of some other known Gramineæ? A. G.

6. *Botanical Necrology and Intelligence.*—We have to record the death of two distinguished botanists, viz, Dr. MOLKENBOER, of Leyden, an accomplished muscologist, the author, in conjunction with Dr. Dozy, of the *Bryologia Javanica*, a work recently commenced, and of which five fascicles have been published. The surviving author, Dr. Dozy, informs us that he shall continue this excellent work, notwithstanding this great calamity, if it shall meet with a sale sufficient to defray the expenses of production. Dr. Molkenboer died in September last, at the early age of 38, deeply regretted by a wide circle of friends.

C. A. MEYER, long a distinguished member of the Imperial Academy of St. Petersburg, and, since the removal of the late Dr. Fischer, the Director of the St. Petersburg Botanic Garden, of late years the principal representative of botanical science at the Russian capital, died on the 25th of February last, at the age of 59 years. It is said that the important position vacated by the death of Dr. Meyer has been offered to Prof. Griesbach of Göttingen, and that he will probably accept it.

A. G.

7. *Description of a New Genus of Crinoidea;* by LUNSFORD P. YANDELL, M.D.*—In a paper on the geology of Kentucky by Dr. Shumard and myself, published in 1847, the figures of several new Crinoidea were given, and among the rest that of the remarkable individual which I am about to describe. It was found in Grayson County, Ky., near some mineral springs which have attracted considerable notice on account of their medicinal qualities. A very slight examination of this Crinoid, I think, will satisfy any palæontologist that it must form a new genus.

ACROCRINUS. (*ακρος*, *extremus*, and *κρινον*, *lilium.*)—Body goblet-shaped, constituted of about 16 series of plates generally hexagonal, and increasing in size from the base to the summit, each series including from 20 to 25 pieces, the last supporting 5 large arm-bearing plates, which give origin to 20 rays composed of a double row of tentaculated joints; pelvis undivided? large, circular, saucer-shaped; column round, consisting of thin, serrated joints gradually expanding towards the base of the cup.

The specimens of this crinoid yet discovered have not enabled us to determine with certainty the position or character of the oral orifice, or the number of pieces composing the pelvis. Its claims to form a new genus rest upon the extraordinary number of series of plates constituting the body from the pelvis to the arms, and the increase of the plates in size as they ascend. To this may probaby be added, an undivided pelvis, in which it agrees with no genus but Platycrinus, which is separated from Acrocrinus as widely as possible by the number, size, and arrangement of its plates.

* Communicated for this Journal.

Acrocrinus Shumardi.—But one species of this crinoid has been found. In this, the plates are thin, smooth, and without ornament. The body of the largest specimen from the pelvis to the rays is two inches in length. Alimentary canal round and diminutive.

The figure represents the specimen described, showing the body flattened and somewhat mutilated, with one ray and a part of the column attached. An interesting feature in this specimen is the presence of a univalve mollusc (*Capulus*) on the summit of the crinoid, in a position to have been grasped by the arms of the living animal. In the paper to which reference has been made, we ventured the suggestion that the encrinite was probably devouring the shell-fish at the time when it perished, but the disproportionate size of the prey to the eater caused a doubt as to the correctness of this opinion. Since that time however we have found

numerous specimens of encrinites with univalve mollusca in a similar position, and now no longer question the truth of our first conjecture.

Locality.—Carboniferous limestone, Grayson County, Ky., associated with Archimedes, Pentremites florealis, Poteriocrinus spinosus, *Owen* and *Shumard*, and numerous undescribed Crinoides.

IV. Astronomy.

1. *Elements of Dien's Comet*, (Compt. Ren., t. 40, p. 636.)—The following parabolic elements of the comet discovered Jan. 14, by Mr. Dien of the Paris Observatory were computed by Mr. Oudemans from the observation at Berlin of Jan. 15, Kremsmunster Jan. 28, and Leyden Feb. 17.

Perihelion passage 1854, Dec. 16·074, M. T. Greenwich.

Long. perihelion,	165° 52′ 47″	Mn. Eqnx.
" asc. node,	238 19 8	1855, Jan. 1
Inclination,	14 10 57	
Log. q.,	0·13588	

Motion direct.

2. *New Planet*, (Astron. Jour., No. 84.)—Mr. Chacornac of the Paris Observatory discovered a new planet on the 6th of April, in the 13th hour of right ascension, which has been named *Circe*. It is of the eleventh magnitude, and if an asteroid, forms the thirty-fourth of the group. A set of elements for this planet has been computed by Lesser from the following observations:

1855,	*h.*	*m.*	*s.*							
April 6,	14	44	14·0	204°	57′	5″·3	−7°	27′	58″·1	Paris.
13,	10	2	54·1	203	36	35·4	6	38	57·2	Vienna, Berlin.
20,	10	49	20·7	202	12	53·3	−5	49	36·6	Berlin.

1855, April 20·4509 M. T. Berlin.

Mean anomaly,	38° 48′ 53″·0	
Long. perihelion,	157 51 19·7	Mn. Eqnx.
" asc. node,	184 1 45·3	1855·0
Angle of excentricity,	6 34 5·0	
Inclination,	5 10 10·5	
Log. semi-axis major,	0·426961	
" Mn. daily motion,	2·909565	

3. *New Planet*, (Astron. Jour., No. 84.)—A thirty-fifth member of the group of asteroids was detected by Dr. Luther in Bilk, April 19, which has been named *Leucothea*. The following positions were obtained by Professor Rümker:

	h.	*m.*	*s.*	Planet's R. A.	Planet's Dec.
1855, April 21	10	13	18·6	180° 57′ 46″·1	−5° 10′ 16″·7 19
22	10	40	13·0	49 21·3	−5 10 45 2

4. *New Comet*, (Astron. Journ., No., 84).—Dr. Schweitzer discovered a telescopic comet at Moscow on the 11th of April, and observed it as follows:

1855 April 11,	184° 40′	−17° 20′
15,	182 20	−13 40

5. *Cause of the Zodiacal Light;* by Rev. GEORGE JONES, Chaplain U. S. N., (Astron. Journ., No. 84.)—In my recent cruise in the U. S. steam-frigate Mississippi, chiefly in the China and Japan Seas, but taking us also around the globe, I had excellent opportunities for observation of the Zodiacal Light. This light, you know, appears to the best advantage within the tropics, where it stretches upward to a great elevation, and is a remarkable object; but it is also very desirable to observe it in high latitudes; and in this also I was favored, as our voyaging extended from 41° north to 53° south latitude, and in some instances our transitions, for weeks together, were very rapid, thus giving me opportunities for observing whether any parallel could be made or not.

I was also fortunate enough to be twice near the latitude of 23° 28′ north, when the sun was at the opposite solstice, in which position the observer has the ecliptic, at midnight, at right angles with his horizon, and bearing east and west. Whether the latter circumstance affected the result or not I cannot say; but I then had the extraordinary spectacle of the Zodiacal Light, simultaneously at both east and west horizons, from 11 to 1 o'olock, for several nights in succession.

In the first part of our cruise my observations were of a desultory character; but I soon began to see the necessity of great precision, and accordingly constructed star-charts from a celestial globe (a small but excellent one) that happened to be on board, which charts I afterwards had cut in wood at Canton, and thus I was furnished with materials for accurately recording all the changes of this phenomenon, not only in successive nights, but also in the successive hours of the same night. My rule was to draw on my chart the boundaries of the Zodiacal Light as exhibited among the stars, with such annotations as the case required; and then, again do the same after an interval of an hour or half-hour; and so to continue, generally as long as the boundaries could be made out reliably; then, if the morning admitted it, to resume the observations at the earliest possible hour, and so to proceed until the dawn. I have thus, in many instances, observations for every hour of the night.

At an early period I began to query whether the moon, when near its full, might not give a Zodiacal Light: and at last when I had gained more experience in observing, and in the peculiar character of this light, I was able to get, at different periods, fourteen reliable observations of what I think must be considered a Zodiacal Light produced by the moon. I have also two records of a distinct Zodiacal Light produced by the joint action of the sun and moon, i. e. at the hour when the moon, then near its first quartering, was about 65° above the western horizon; the reflection from the combined light of the sun and moon being sufficient to overpower the moonlight proper, and thus to produce a decided stream of light in the sky within the Zodiacal-Light boundaries. The latter of these observations was the more remarkable inasmuch as the moon was then *without* the boundary of this reflected light.

You will excuse my prolixity in stating these varieties of observation, for the conclusion from all the data in my possession is a startling one. It seems to me that these data can be explained only by the

supposition of a nebulous ring with the earth for its centre, and lying within the orbit of the moon. This conclusion seems to evolve itself, —1st, from the simultaneous midnight east and west observations, which preclude the possibility of a ring around the sun *within* the earth's orbit; 2d, from the great hourly lateral changes (often semi-hourly) in the boundaries of the light caused by the observer's change of place, in that time, as regards the ecliptic or axis of the Zodiacal Light, which lateral change in the light is too great to allow of our considering it at a distance of 170,000,000 of miles, as its lower end would be, near dawn, if it is a ring around the sun and beyond the earth's orbit; and 3d, from the moon's Zodiacal Light, if real, which I think it is. That it is a ring, the unbroken continuity of my observations satisfactorily determines. For more than two years I never failed to see this Light, evening and morning, when the moon and clouds did not interfere: and, except one evening, I have continuous records of this kind.

I could get no parallax; but, on the contrary, as we went south, the boundaries of the Zodiacal Light changed with us to the south among the stars; and so *vice versa*, towards the north, caused, doubtless, by the ring's presenting new portions of its wide reflecting surface to the sun's light.

V. Miscellaneous Intelligence.

1. *Contributions to Meteorology.—Mean results of Meteorological Observations made at St. Martin, Isle Jesus, Canada East,* (nine miles west of Montreal,) *for* 1854; by Charles Smallwood, M.D.—The geographical co-ordinates of the place are 45° 32′ N. Lat., and 73° 36′ W. Long. Height above the level of the sea, 118 feet.

Barometric Pressure.—The readings of the barometer are all corrected for capillarity, and reduced to 32° F. The whole of the means are obtained from three daily observations taken at 6 A. M., 2 P. M., and 10 P. M.

The mean height of the *barometer* in January was 29·516 inches, in February 29·520, in March 29·024, in April 29·440, in May 29·731, in June 29·814. in July 29·916, in August 29·910, in September 30 001, in October 29·949, in November 29·764, in December 29·540 inches. The highest reading was on the 23d of December, and indicated 30·954 inches; the lowest was in March, on the 16th day and was 28·607, inches; the yearly mean was 29·677 inches; the mean yearly range was equal to 1·907 inches. The *atmospheric wave* of November was marked by its usual fluctuations, the final trough terminated on the 25th day.

Thermometer.—The mean temperature of the air, by the *standard thermometer*, was in January 10° 92, in February 12° 20, in March 25° 84, in April 37° 75, in May 57°·17, in June 63° 80, in July 76° 20, in August 68° 31, in September 58° 01, in October 48° 40, in November 32° 99, in December 7° 35. The highest reading of the *maximum thermometer* was on the 20th of July and marked 100° 10, the lowest reading of the *minimum thermometer*, was on the 23d of December and was —36° 20 (below zero). The mean temperature of the quarterly periods was, Winter 13° 26, Spring 40° 25, Summer 69° 43, Autumn 46° 46. The *yearly mean* was 41° 57, and the mean yearly range 136° 30.

The greatest intensity of the sun's rays was in July, and indicated 140° 2, the least intensity was in December, and was 58° 0, and the lowest point of terrestrial radiations was −36° 9, (below zero) in December.

The mean humidity (saturation being 1·000) was, in January ·843, in February ·825, in March ·840, in April ·835, in May ·723, in June ·780, in July ·709, in August ·714, in September ·781, in October ·874, in November ·878, in December ·850. The yearly mean was ·804, which was *minus* ·021 of last year.

Rain fell on 80 days; it was raining 231 hours 16 minutes, and amounted to 40·505 inches, and was accompanied by thunder and lightning on 14 days. I observed no yellow matter in the rain, which fell this year. The amount of rain which fell in January was 1·067 inches, in February 0·150, in March 0·910, in April 7·886, in May 3·418, in June 8·384, in July 0·174, in August 2·265, in September 6·167, in October 4·844, in November 5·130, in December 0·110 inches.

Snow fell on 53 days, it was snowing 222 hours 6 minutes, and amounted to 97·45 inches on the surface. The monthly fall was as follows: in January the amount was 17·98 inches, in February 23·96, in March 28·61, in April 4·03, in October 3·10, in November 1·10, in December 18·67 inches. The first snow of the winter 1853–4 fell on the 15th day of October 1853, and the last fell on the 30th day of April 1854; the whole amount of snow which fell in the winter 1853–4, amounted to 116·81 inches. The river Jesus was frozen over on the 28th day of November. The last steamer left Montreal (on the St. Lawrence) on the 7th of December 1853; the first steamer arrived at Montreal on the 22d day of April 1854. The winter 1853–4 fairly set in on the 17th day of December.

The amount of evaporation was measured regularly from the 1st of April to the 31st of October, and amounted in April to 1·86 inches, in May 4·13 inches, in June 2·95 inches, in July 5·12 inches, in August 4·70 inches, in September 3·11 inches, in October 1·49 inches. This period includes what I consider could be taken with anything approaching to accuracy, owing to frosty weather. The greatest amount of evaporation I have observed here, was on the first six days of June and amounted to 1·00 inches. The month of July was remarkable for its dryness the amount of evaporation in the month was 5·12 inches, and the amount of rain which fell *amounted only* to 0·174 inches, the temperature was 8° 2, above that of last year. Very few birds were seen in the fields, owing to the scarcity of water, and vegetation suffered very much from the unusual drought. The mean humidity of the month was ·709, while that of the corresponding month of last year was ·727, and of 1853, ·845.

The most prevalent wind, during this year was the N. E. by E.; the least prevalent was the S. S. E. In the winter quarter the most prevalent wind was the N. E. by E. and the least S. In the spring quarter the most prevalent wind was the N. E. by E. and the least S. S. E. In the summer quarter the most prevalent wind was the S. W. by W. and the least so the E. In the autumn quarter the most prevalent wind was W. S. W. and the least E. The greatest velocity of the wind was on the 4th day of December, and was 40·17 miles per hour, the yearly

mean of the *maximum velocity* was 19·53 miles per hour, the yearly mean of the *minimum velocity* was 0·16 miles per hour. The quarterly means were as follows: winter maximum velocity 17·06, minimum velocity 0·12; spring maximum velocity 19·86, minimum velocity 0·21; summer maximum velocity 55·51, minimum velocity 0·00; autumn maximum velocity 13·80, minimum velocity 0·31 miles per hour.

Crows wintered in this place although the winter was very severe, wild geese, *Anser canadensis*, were first seen on the 20th day of April; swallows, *Hirudo rufa*, were first seen on the 12th of April; shad, *Alosa*, were first caught in this neighborhood on the 30th of May; fire-flies, *Lampyris corusca*, were seen on the 12th day of June; frogs, *Rana*, were first heard on the 2nd of May. The "*Rossignol*" (the harbinger of the Canadian spring) was said to be heard on the 16th of March, and snow-birds were last seen on the 27th of April.

The Aurora Borealis was visible on 50 nights as follows:

January 8th, 2.40 A. M. Faint auroral light.—23d, 10 P. M. Low auroral arch faint.—28th, 6.30 P. M. Dark segment at the horizon, arch distinct and bright, occasional streamers; a second arch was visible passing from west to east through the constellation "*Taurus*;" 6.55, the upper arch less distinct; 9.15, auroral light diffuse; 10.0, upper arch vanished, frequent and brilliant streamers to the zenith; 11.0, lower arch very bright and less diffuse.—29th, 8.0 P. M. Faint auroral light in the horizon; 9.0, idem, dark segment at the horizon; 10.0, idem. Aurora might have been seen on 11 nights this month.

February 10th, 10 P. M. Very faint auroral light.—11th, 4 A. M. Bright auroral arch at the horizon, frequent streamers.—16th, 9 P. M. Brilliant arch, dark segment at the horizon, auroral light of a bright, white color; 10 P. M., dark segment vanished.—24th, 7 P. M. Dark arch at the horizon, surmounted by a very bright arch of auroral light; 9 P. M., arch extended to near the zenith of a very pale color and extending from east to west.—27th, 10 P. M. Two distinct arches of auroral light, patches of auroral clouds in the west, occasional streamers of a green and yellow color. *Zodiacal light* very bright during the month. Aurora might have been seen on ten nights.

March 6th, 10 P. M. Faint auroral light at the horizon.—12th, 10 P. M. Lunar halo, diam. 35° 6′.—16th, 10 P. M. Heavy *cumulo-stratus* at the horizon, auroral light visible above them.—20th, 10 P. M. Very faint auroral light.—21st, 7.45 P. M. Auroral arch stretching from E. to W., and passing to the horizon forming the base of the *zodiacal light*, which is very bright and well defined; 10 P. M., very faint.—29th, 9 P. M. Patches of auroral clouds in W. et W. N. W., and E. None in the N., or if any, very faint.—30th, 10 P. M. Faint auroral light. Aurora might have been seen on 7 nights.

April 3d, 8 P. M. Comet visible.—6th, 8 P. M. Comet still visible.—10th, 9 P. M. Lunar halo, diam. 60°.—14th, 10 P. M. Very faint auroral light at the horizon.—15th, 10 P. M. Auroral arch 2° wide, stretching from the eastern to the western horizon, of a pale yellow color, passing through the constellations "*Cassiopeia*" and "*Taurus*." —21st, 9 P. M. Bright auroral light in the N.; 9.36, an arch of auroral light passing a little south of the zenith through "Leo," and stretching from E. to W., the eastern limit somewhat distorted; 9.40,

arch vanished; 10.0, auroral light still in the N.—23d, 10 P. M. Faint auroral light at the horizon. 24th, 10 P. M. Idem. Aurora might have been seen on 8 nights.

May 4th, 10 P. M. Faint aurora in the N.; dark segment underneath.—16th, 9.30 P. M. Patches of auroral clouds forming an imperfect arch in the zenith, faint auroral light at the horizon.—23d, 10 P. M. Faint auroral arch, dark segment underneath, frequent streamers from the eastern limit.—26th, 10 P. M. Faint auroral light to the horizon.—28th, 9.45 P. M. Very faint auroral arch in the zenith passing from E. to W., lasting only a few minutes. Aurora might have been seen on 15 nights.

June.—Aurora Borealis was visible on 2 nights, but very faint. Might have been seen on 13 nights.

July 3d, 11 P. M. Auroral arch of moderate brightness, frequent streamers.—5th, 10 P. M. Faint auroral light.—12th, 10 P. M. Idem and occasional streamers.—14th, 10 P. M. Faint light, dark segment underneath.—27th, 29th and 30th, 10 P. M. Very faint light at the horizon. Aurora might have been seen on 14 nights.

August 17th, 10 P. M, Auroral arch of moderate brightness at the horizon.—20th, 10 P. M. Auroral light faint, dark segment underneath.—25th, 10 P. M. Bright auroral light, frequent streamers. Aurora might have been seen on 15 nights.

Sept. 10*th*, 10 P. M. Faint aurora, frequent streamers, violet color. 12th, 10 P. M. Auroral light to the horizon, of moderate brightness.—13th, 10 P. M. Faint auroral light.—15th, 10 P. M. Idem.—17th, 10 P. M. Faint auroral light, dark segment underneath.—20th, 10 P. M. Two distinct arches of faint auroral light of about 3°; dark band between them of 8° broad.—21st, 10 P. M. Auroral light, dark segment underneath.—26th, P. M. Faint auroral light. Aurora might have been seen on 10 nights.

Oct. 18*th*, 10 P. M. Faint auroral light at the horizon.—25th, 10 P. M. Idem. Aurora might have been seen on 12 nights.

November.—Aurora borealis invisible this month. Might have been seen on 8 nights.

December.—Faint aurora borealis on the 8th, 18th and 22d. Might have been seen on 10 nights.—26th, 10 P. M. *Lunar halo*, 45° 6′.

Zodiacal light very bright this month.

Electrical state of the Atmosphere.—The atmosphere has daily afforded indications of electricity, but of less intensity than that of last year.

Ozonometer.—Observations of the amount of ozone are still registered twice daily, and as might be supposed from the dryness of the year, indicated a less amount than for the last year, for the presence of moisture in the atmosphere generally shows an increase.

St. Martin, Isle Jesus, C. E., January, 1855.

2. *Microscopes of American Manufacture.*—A few years since, only five or six, it was a thing impossible to obtain in the United States a microscope of any value of domestic manufacture. Thanks to the genius of Spencer and the Messrs. Grunow of New Haven, this is now quite possible. The merit of Mr. Spencer in this department of optical art, the clearness of definition, power of illumination, absence of color

and breadth of angular aperture, in his higher powers are well known and have been long since acknowledged—even by his European competitors. In the New York Crystal Palace Exhibition the first prize for Microscopes was awarded to Mr. Spencer, the second to Messrs. Grunow. Since that time the latter makers have steadily advanced in the excellence of all parts of their instruments. Their form of stand for steadiness, ease of management, simplicity and economy is unrivalled —and the moveable stage with universal motion in one plane by one lever, improved and admirably executed by them, is now universally adopted in the best American instruments. Their brass work is perfect in all respects. One of the most experienced microscopists in the United States writes as follows, of the optical value of their objectives: "I have tried their $\frac{1}{8}$th objective, and it compared so well with my Spencer $\frac{1}{8}$th that I consider them of equal value. I have also a $\frac{1}{4}$th inch objective made by them which works quite as well as the best English glasses. I need not tell you how much I rejoice that so worthy a man as Mr. Grunow has succeeded so well as an optician. His students microscope I have been familiar with for some time. It is a source of congratulation that our students of medicine can now obtain the means of pursuing microscopic studies at a reasonable cost, and without the vexatious delays we have experienced in importing European instruments."

The Inverted microscope invented by Dr. J. L. Smith, is built only by them and in a very superior style of workmanship. They also produce very perfect geodesic and surveying instruments, and are prepared to undertake any instrument of research or illustration required in refined physics or chemistry. We take pleasure in calling the attention of scientific men generally to the establishment of Messrs. Grunow, as adding a new and valuable resource to their means.

We have recently received a circular, from Wm. Buffhum & Son, makers of Microscopes and Telescopes at Milburne, Lake Co., Illinois.

3. *Melloni.*—An oversight occurs in M. Nicklès's obituary notice of the distinguished physicist, *Melloni.* He states that Melloni's "treatise, entitled, *La Thermochrosie*, was found among his papers, and published by his scientific friends." This can only refer to a second part of that interesting work. The first volume was published at Naples in the year 1850, and was well known to the physicists of this country during the author's life-time. The writer of this had the pleasure to receive a copy from Melloni a year ago.

4. *On the Application of Photography to Astronomical Observations*; by Sir John F. W. Herschel, (Proc. Astronom. Soc. in Athen., No. 1435.)—I consider it an object of very considerble importance to secure at some observatory, and indeed at more than one, in different localites, daily photographic representations of the sun, with a view to keep up a consecutive and perfectly faithful record of the history of the spots. So far as regards the general delineation of the whole disk, and the marking out on it, in reference to the parallel to the equinoctial passing through its centre, the places, sizes, and forms of the spots, there would need, I should imagine, no very powerful telescope—quite the contrary; but it should be equatorially mounted, and ought to have a clock motion in the parallel. The image to be impressed on the paper (or collodionized glass) should be formed not in the focus of the object-lens, but in that of the eye-lens, drawn out somewhat beyond the

proper situation for distinct vision (and always to the same invariable distance to insure an equally magnified image on each day). By this arrangement, a considerably magnified image of the sun, *and also of any sysiem of wires* in the focus of the object-glass, may be thrown upon the "focussing-glass" of a camera-box adjusted to the eye-end of a telescope. By employing a system of spider-lines, parallel and perpendicular to the diurnal motion, and so disposed as to divide the field of vision into squares, say of 5′ in the side, the central one crossing the sun's centre (or rather, as liable to no uncertainty, one of them being a tangent to its lower or upper limb), the place of each spot on the surface is, *ipso facto*, mapped down in reference to the parallel and declination circle and its distance from the border, and its size measurable on a fixed scale. If large spots are to be photographed specially with a view to the delineation of their forms and changes, a pretty large object-glass will be required, and the whole affair will become a matter of much greater nicety; but for reading the daily history of the sun, I should imagine a 3-inch object-glass would be ample. The representations should, if possible, be taken daily, and the time carefully noted. As far as possible, they should be taken at the same hour each day; but in this climate, a clear interval, occurring when it may, had better be secured early in the day. Three or four observations in tropical climates, distant several hours in longitude (suppose 3, at 8^h distance in longitude), each recording at or nearly at noon, would, when the results were assembled, keep up a continuous history of the solar disk. With regard to proper preparation of paper, or the use of collodion, the photographic art is now so much advanced, that no difficulty can arise in fixing upon fitting preparations, or the manipulations necessary for multiplying them. But it would be very requisite that many impressions of each day's work should be taken and distributed, and an interchange kept up among observers.

5. *Preservation of Insects.*—We have received from Mr. Wm. S. Van Duzee of Buffalo, N. Y., a specimen of a method which he has devised for the arrangement and preservation of a cabinet of insects. He proposes to take glass-stoppered jars with a large mouth, attach to the under side of the stopper a rectangular strip of cork or soft board (whitened with paper or otherwise) as broad as the stopper and as long as the jar will hold; then arrange the insects on one side of this upright piece. The insects thus arranged would show finely in a collection,—could be always on shelves for exhibition—would be perfectly secure from insects, even without camphor, though it may still be used,—and the jars could be easily opened and insects taken out for special examination. Moreover, he suggests that the insects may be numbered, and on the opposite side of the board the names of the species may be given.

6. *Geological Survey of Great Britain*, (Athen., No. 1437.)—Sir R. I. Murchison has received the appointment of Director of the Geological Survey,—the post left vacant by the death of Sir Henry De la Beche,—and the salary of which is returned in the Estimates as £800.

7. *American Association for the Advancement of Science.*—The next annual session of the American Association will be held in Providence, Rhode Island, in August next, commencing with the third Wednesday of the month. Dr. John Torrey is the President for the ensuing year.

8. *Anesthesis.*—The Imperial Decoration of the "Majidieh" of the fifth class, a Decoration named after the Sultan, the Founder, has been conferred by the Sultan of Turkey on Dr. C. T. Jackson of Boston, as an expression of the Sultan's "estimation of his discoveries, and a mark of his Imperial favor."

9. OBITUARY.—*Sir Henry Thomas de la Beche*, (Ath., April 21, 1854.)—SIR HENRY DE LA BECHE was an example of that rare combination, a man of science and a man of the world. He succeeded in obtaining the end at which he aimed; and he has left in the Geological Survey and the Museum of Practical Geology enduring monuments. A notice of the labors of such a man,—who was thoroughly practical before the commencement of this practical age, and who aimed at educating the people in science long before the Great Exhibition made scientific education a fashion,—cannot but be of interest.

Henry Thomas De la Beche was the eldest son of Col. De la Beche: his family being descendants from the Barons De la Beche, who were settled at Aldworth, Berks, in the time of Edward the Second. He was born in London in 1796; but his youth was passed amidst the lovely valleys of Devonshire: his first education having been received at the School of Ottery Saint Mary. There is little doubt that the geological tendencies which were subsequently developed were due to the contemplation of nature in this locality and in the scenes around Charmouth and Lyme Regis—rich in organic remains,—which places were for some time the residences of his parents.

In 1810, Mr. De la Beche entered the Royal Military College, then at Great Marlow, but afterwards removed to Sandhurst; on leaving which he entered the army: but in a little time he resigned the profession of arms for the pursuits of science. For a man of wealth and fashion to devote himself to any study was in those days a phenomenon; and the adoption of a science then in its infancy and struggling into life, through the prejudices of the ignorant and the timid, was not a little remarkable.

Mr. De la Beche, however, gave himself up to the study of Geology and made it the business of his life. In 1817 he became a member of the Geological Society, then in the tenth year of its existence. In 1818 he married the daughter of Capt. Charles White, of Lough Brickland, County Down, Ireland, who died in 1844, leaving one daughter.

The year 1819 was spent by Mr. De la Beche in an examination of the geological formations of Switzerland and Italy, and his zealous prosecution of similar inquiries led to his being elected in that year a Fellow of the Royal Society. In 1820 a paper by Mr. De la Beche, "On the Temperature and Depth of the Lake of Geneva," the result of a most careful examination, was published in the Edinburgh Philosophical Journal. In his geological investigations of the British rocks the Rev. Wm. Conybeare, now the Dean of Llandaff, was, to some extent, connected with Mr. De la Beche; and his first communication to the Geological Society was the joint prodution of these two geologists,—announcing the discovery of a new fossil animal of the Saurian family, in the lias limestones of Bristol, which they named, as being distinctive of its species, the Plesiosaurus. From this time the name of De la Beche became closely connected with the science of the day. Many

valuable papers were communicated to the Geological Society, including an elaborate account of the Geology of Switzerland; the Fossil Plants found at the Col de Balme, near Chamouny; a communication on the Geology of the Coast of France; and several papers on the Geology of various districts in the British Isles,—especially of Southern Pembrokeshire, of Lyme Regis, Dorsetshire, and of Beer in Devonshire.

Mr. De la Beche possessed extensive estates in Jamaica. He now visited his property,—Halse Town, in the neighborhood of Spanish Town, and on his return, in 1825, he communicated to the Geological Society his remarks on the geology of that West Indian Island, of which nothing had been previously known.

Between 1827 and 1830, Mr. De la Beche, published numerous important geological papers in the Transactions of the Society, the Philosophical Magazine, and the Annals of Philosophy, and also a tabular proportional view of the superior, supermedial, and the medial rocks. In 1830 his first book, "Geological Notes," appeared; and in the same year, "Sections and Views of Geological Phenomena." Great skill in the use of the pencil enabled the author to furnish the whole of the drawings for these works, and to them all subsequent illustrators have been indebted. "The Geological Manual" was published in 1831, and was speedily translated into French and German,—becoming a text-book for geologists throughout Europe, and passing through several editions. In 1832 Mr. De la Beche proposed to the government to supply the data for coloring geologically the maps, then in progress of publication, of the Ordnance Trigonometrical Survey. This offer was accepted, and at the Land's End, in Cornwall, was commenced the great work of this eminent geologist's life. Mr. De la Beche, who bore himself the greater part of the expense of the Geological Survey of Cornwall, devoted several years to a careful investigation of all the conditions, lithological and mineralogical, of Western England; and he published a series of maps of Cornwall, Devonshire, and Somerset, which exhibited a correctness and detail such as had never before been attained. This Survey was fairly established under the Ordnance. "It was,"—says Sir Henry De la Beche, in his Inaugural Discourse, delivered at the opening of the School of Mines, on the 6th of November, 1851,—"It was while (in 1835) conducting the Geological Survey then in progress, under the Ordnance in Cornwall, that being forcibly impressed that this Survey presented an opportunity not likely to recur, of illustrating the useful applications of geology, I ventured to suggest to Mr. Spring Rice (now Lord Monteagle), then Chancellor of the Exchequer, that a collection should be formed, and placed under the charge of the Office of Works, containing specimens of the various mineral substances used for roads, in constructing public works or buildings, employed for useful purposes, or from which useful metals were extracted, and that it should be arranged with every reference to instruction; as by the adoption of this course, a large amount of information, which was scattered, might be condensed, and those interested be enabled to judge how far our known mineral wealth might be rendered available for any undertaking they are required to direct, or may be anxious to promote, for the good or ornament of their country."

Being supported in this recommendation, the nucleus of the Museum of Practical Geology was formed in an apartment in Craig's Court. This collection in a short time filled one house; and even when the Earl Marshall's Office adjoining was added to it, the Museum, by the exertions of its founder, was soon found to outgrow these buildings. A Laboratory was added to the Museum, and placed under the care of the late Richard Philips. The business of the Geological Survey was greatly extended, and the Palæontological department was superintended by the late Edward Forbes. The Mining Record Office was also, at the recommendation of the British Association, united to the Museum. In 1839, the sanction of the Treasury was obtained for Lectures on Geology, and its associated sciences, in their application to the useful purposes of life. Owing to the deficiency of room, it was not possible to commence these lectures until 1851; when the building in Jermyn Street received the valuable collections of the Museum and furnished the theater in which Sir Henry de la Beche delivered the Inaugural Address from which we have quoted.

In 1848, the honor of knighthood was bestowed on the Director of the Geological Survey; and in addition to this honor, in 1853 Sir Henry de la Beche was elected, by the suffrages of forty-seven members, Corresponding Member of the Academy of Sciences of Paris. The Order of the Dannebrogg was bestowed on him by the King of Denmark; and he received the Order of Leopold from the King of the Belgians.

Beyond the works and papers which we have enumerated, Sir Henry de la Beche published a voluminous report on the 'Geological Survey of Cornwall, Devonshire, and West Somerset,' 'Researches in Theoretical Geology,' and 'How to Observe.' In the various journals will be found forty papers and memoirs; and in 1851 Sir Henry de la Beche completed his last work, 'The Geological Observer,' founded upon his former work 'How to Observe.' In all these productions will be discerned a minuteness of detail and an excellence of illustration which mark the rare union of a skillful scientific observer and a finished illustrative draughtsman.

Although paralysis was seen by his anxious friends to be slowly but surely spreading its fatal influences over his once energetic frame, Sir Henry de la Beche would not allow himself repose. The labors of the Geological Survey and the business of the Museum engaged his attention daily,—and even two days before his death he spent several hours in the Museum directing the' business of that establishment with his usual acuteness, although then powerless to move himself.

Sir Henry de la Beche raised for himself a splendid memorial of his talents and his zeal, and he created for the public an establishment which cannot but prove eminently useful, if it be carried onward in the spirit and with that well-defined idea—which has been the creative power and the sustaining influence—under which the Museum of Practical Geology and the School of Mines were formed and have been supported.

G. B. Greenough.—Seldom does it fall to our share of melancholy duty to record two such losses in one week as De la Beche and Greenough. The two men had something in common besides devotion to a common

pursuit and the coincidence of their deaths. Both were men of fortune. Both were designed for a worldly career. Both abandoned more ambitious schemes in favor of science. Both achieved solid reputation. Both were hard workers as well as clear thinkers; and they enjoyed in common that faculty for organization which is rarer in Englishmen than the faculty of observation. The Geological Society is the monument of Mr. Greenough, as the Museum of Practical Geology is that of Sir Henry De la Beche.

Mr. Greenough was born in 1778, and consequently was seventy-seven when he died, was eduated at Cambridge and Göttingen, and served in Parliament for the famous, or infamous, borough of Gatton. But he made no great figure in the House of Commons. His genius was a genius for map-making, not for speeches and legislation; and the records of his zeal which remain to tell posterity of his useful labors are 'The Geological map of England and Wales,' the map of 'Hindüstan,' and the 'General Sketches of the Physical Features of British India.' He was on a journey to the East, in hopes of collecting materials for new maps, when he died at Naples: and it is understood that he has left behind a vast accumulation of materials, some of which will doubtless be available for the press.

Mr. Greenough had a great reputation among men of science, without being very widely known to the British public. He was a worker, not a writer. More than thirty years ago he published his one volume, 'A Critical Examination of the First Principles of Geology.' Addresses and discourses to scientific societies followed from time to time; but not with that persistence of assault by which literary fame can alone be carried. Yet was Mr. Greenough considered by English geologists as the leader of their band, and he was one of the founders and was the first President of the London Geological Society.—*Athen.*, April 21.

10. *A Treatise on the Differential and Integral Calculus, and on the Calculus of Variations;* by Edwd. H. Courtenay, LL.D.—This work, though not published till after the death of its accomplished author, has been written with great care. No work on the Transcendental Analysis has issued from the American press, since the republication of Young's Treatises, so full and extended as this. A noticeable peculiarity of it, among American and English works on Analysis, is the absence of all examples for practice. Every principle is copiously illustrated by examples wrought out at length by the author; but beyond this no examples swell the pages of the work. Students will be much obliged to the author for the simple manner which he exhibits (p. 55,) of finding the successive differential coefficients in the applications of Maclaurin's theorem, without the labor usually employed.

The chapters on Maxima and Minima, that on the Integration of Trigonometric and Circular functions, and the one on Singular Solutions, will attract the attention of every reader by their fullness and perspicuity.

A. R.

11. *Practical Meteorology;* by John Drew, Ph.D., F.R.A.S. 290 pp., 12mo. London, 1855. John Van Voorst.—This small volume was prepared in order to give to those who may be interested in practical meteorology, such information upon the general principles of meteorology in its different departments, and on the nature, choice and use

of instruments, as would be a sufficient guide to the learner as well as a help to the more experienced. Its object therefore is to teach "how to observe" meteorological phenomena. Besides practical and simple theoretical observations, there are good figures of meteorological instruments on copper-plates through the volume, and also a number of tables, such as, for the Tension of Aqueous Vapor; Comparison of Thermometric Scales; Degree of Humidity from Readings of the Dry and Wet Bulb Thermometer; Measuring Heights with the Barometer; Weight of a cubic foot of air saturated with different degrees of moisture, etc. Dr. Drew is a member of the Council of the British Meteorological Society.

12. *Report on the Agriculture and Geology of Mississippi, embracing a sketch of the Social and Natural History of the State*; by B. L. C. WAILES, Geologist of Mississippi. 1854.—This handsome octavo volume of 371 pages, with a map and other illustrations, is published by order of the Legislature of the State. The heads are: 1, Historical Outline; 2, Land Titles; 3, Agriculture; 4, Geology; 5, Fauna; 6, Flora, with an Appendix.

The means provided for the publication being inadequate, the author made a partial abridgment and was constrained to omit most of the plates.

Although the work was therefore for some other reasons executed under disadvantages, it does honor to the skill and industry of Col. Wailes who has been long known as an active cultivator and devotee of science. The historical sketch occupies 125 pages, the agricultural 79, the geology 90, and other topics of natural history 60 pages more. If the civil history occupies one-third of the volume it is information of interest to the State. We learn from it that the possession of that fine country was not obtained without severe vicissitudes, and distressing struggles continued through almost two centuries in which the French and Spanish and English nations and powerful tribes of aborigines, and finally the Anglo-Americans were active agents. The portion on agriculture is instructive, especially in relation to local interests. That on geology contains interesting facts in relation to palæontology, drift, fountains, lignite, &c.

We trust that the legislature of Mississippi having begun so well, will not hesitate to encourage their able and active geologist in extending and perfecting his researches which have thus far redounded to the honor and substantial advantage of the State in its most important material and social interests. B. S. SR.

13. *Natural History of the United States.*—The following Prospectus has been issued by Prof. AGASSIZ with reference to a great American work which he has commenced. All lovers of science through the land will rejoice in this prospect of the publication of Prof. Agassiz's researches.—EDS.

Contributions to the Natural History of the United States, in ten vols. quarto; by LOUIS AGASSIZ. To be published by Messrs. Little, Brown and Co. of Boston, Mass.—For more than eight years, I have now been in this country, devoting my attention chiefly to the study of those classes of the Animal Kingdom which American naturalists have, thus far, not fully investigated. The amount of materials I have

already brought together is so great, that the time seems to me to have come when I should proceed with the publication of the more important results of these investigations. Desirous of contributing my share to the rapid progress natural sciences are making at present in this part of the world, I wish to present my work to my fellow-laborers in this field in the form most easily accessible to them. It has therefore appeared to me desirable to bring it out in a series of independent volumes. This plan will, moreover, leave me entirely free to present my contributions to science with such minute details, and to such an extent, as I shall deem necessary to the fullest illustration of my subject.

Without entering into a detailed account of the contents of this work, it may be sufficient here to state, that it will contain the results of my embryological investigations, embracing about sixty monographs, from all the classes of animals, especially selected among those best known as characteristic of this continent; also descriptions of a great number of new genera and species, accompanied with accurate figures, and such anatomical details as may contribute to illustrate their natural affinities and their internal structure.

I shall not extend my publications to classes already illustrated by others, but limit myself to offering such additions to the Natural History of the States I have visited as may constitute real contributions to the advancement of our knowledge.

From a careful estimate of the materials I have now on hand, I am satisfied I shall be able to include the most valuable part of my investigations in ten quarto volumes; each volume containing about 300 pages, with at least twenty plates. I therefore now open a subscription for such a work, in ten volumes, quarto, in cloth binding, at the price of twelve dollars each volume, payable on delivery. Each volume shall be complete in itself, containing one or several independent monographs; so that if any unforeseen difficulties should interrupt the publication of the whole, the parts already published shall not remain imperfect. As far as possible, I shall always select first such of my papers as contain the largest amount of new matter, or may contribute most directly to the advancement of science. Having devoted the greatest part of my time to the investigation of the embryonic growth of our animals, I shall make a beginning with the embryology of our turtles, several of which I have traced through all their changes. I trust this monograph will afford our medical students a fair opportunity of making themselves familiar with the modern results of one branch of physiology, which has the most direct bearing upon their science, and for which the different species of the family of turtles found in every part of the United States will afford them a better opportunity even than the artificial breeding of hens' eggs. Moreover, the extent of my embryological researches, covering as they do, all the classes of the animal kingdom, will furnish, I trust, a new foundation for a better appreciation of the true affinities, and a more natural classification, of animals. I foresee the possibility, upon this basis, of determining, with considerable precision, the relative rank of all the orders of every class of animals, and of furnishing a more reliable standard of comparison between the extinct types of past geological ages and the animals now living upon earth.

I shall have frequent opportunities of acknowledging the many favors I have received from naturalists of all parts of the country, from the Atlantic to the Pacific coast, and from the shores of our great Lakes to those of the Gulf of Mexico; and also of mentioning the many specimens which have been furnished to me from every part of the Union, and of which I shall publish descriptions.

It is a matter of course, that a work like this, illustrated by a large number of plates, cannot be published without a liberal and extensive patronage. As it has been prepared solely with the view of throwing additional light upon the wonderful diversity of the animal creation of this continent, its structure, and its general relation to that of the other parts of the world, without the slightest hope of compensation for myself, I trust I may meet with the approbation of those conversant with the importance of the subject, and receive sufficient encouragement from the enlightened part of the community to enable me to bring to a successful close an undertaking upon which I enter now, and in this form, for no other purpose than to contribute my share towards increasing the love of nature among us.

As the printing of this work cannot begin until a sufficient guarantee is secured for the publication of the whole, I take the liberty of making an appeal to the lovers of science to send to the publishers their own subscriptions, and such others as they may procure, as soon as convenient, and, if possible, before the first of August next, that I may be able to proceed at once with a work, which, relating to animals peculiar to America, I wish to make, in every respect, an American contribution to science, fostered and supported by the patronage of the community at large.

To render this work more generally accessible, it is intended to publish at the rate of about one volume a year. Such an arrangement will bring the whole within the reach of every student of Natural History, and of every friend of the progress of science in the country. The periods of publication, however, cannot be more definitely fixed, because the required uniformity of execution of the plates, to which particular attention will be paid, will demand that they be all entrusted to the same artist, who has drawn on stone most of the plates of my former works.

14. *Report on Crustacea;* by JAMES D. DANA.—The Atlas of the Report on the Crustacea collected in the Exploring Expedition around the world under Capt. Wilkes, U. S. N., has just been issued. It contains 96 plates in folio, containing figures and details relating to nearly 700 species, from drawings by the author. The figures are partly colored: as an introductory note states, a large part of the author's original drawings were unfortunately destroyed by a fire in Philadelphia, after the engravings were completed but before any copies had been colored, and on this account the number of colored figures in the Atlas is comparatively small. The engravings are well executed on copper. The Text of the Report, which was issued near three years since consists of two volumes quarto, extending in all to 1620 pages.

15. *Recherches sur les Crinoïdes du Terrain Carbonifère de la Belgique;* by L. DE KONINCK and H. LE HON, with a notice on the genus Woodocrinus, by L. DE KONINCK, D.M., 218 pp., 4to, with 8 litho-

graphic plates. Bruxelles, 1854.—The labors of Prof. de Koninck have largely contributed to our knowledge of the Fauna of the Carboniferous Period. These researches on the Crinoids are carried forward with the same thoroughness and precision as his former labors. The work opens with a catalogue of all the works or memoirs which have been published upon Crinoids, commencing with Agricola, in the 16th century. It continues with historical and critical observations, an account of their structure and the nomenclature of the parts, and then enters upon the descriptions of species.

16. *Ueber die Gattungen der Seeigellarven.*—Seventh memoir on the Metamorphosis of Echinoderms; by JOH. MULLER. 56 pp., 4to, with 9 copper plates. Berlin, 1855.—From the Transactions of the Royal Academy of Sciences at Berlin.—This is a continuation of the author's elaborate and beautiful researches on the Echinoderms. The plates are admirable.

C. LYELL: Report on the New York Industrial Exhibition, 50 pp. fol. *London*, 1854.—This Report gives a general review of the geology of this country, and of its economical products, and was written in the main by Mr. James Hall, of Albany.

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GRAILICH and PEKÁREK: Das Sklerometer.—An instrument for measuring the hardness of crystals, 30 pp., with a plate. Vienna Sitzungsb., 1854.

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I could ascertain, are the only ancient rocks, as they are the only mountains in the island.

On the south the mountains sink abruptly down nearly to the level of the sea, leaving the remainder of the island an almost unbroken plain of tertiary clay, sandstone and marls.

From the city of Trinidad or Port au Spain, which stands between the base of the hills and the sea, to the Pitch Lake 39 miles south, only a single hill, some two hundred feet high, breaks the uniform, and for the most part, densely wooded surface.

A fine steamer plies up and down the gulf of Paria, touching twice a week at the village of La Braye in front of the Pitch Lake. As we approach the village a long black line between the white foam of the waves and the dark green of the dense vegetation indicates something unusual in the nature of the shore. A small boat bears the passenger through a considerable surf and lands him on rough jagged rocks of hardened, but where the sun touches them, still flowing pitch.

The village of La Braye stands on a projecting tongue of land which owes its preservation from the inroads of the sea to the fact that it consists entirely of hardened pitch, which withstands the waves far better than the loose materials of the accompanying formations. The shore for miles both north and south consists mainly of the same material and juts out boldly into the sea wherever it is thus pitch-bound.

The village itself is built upon pitch. The inhabitants complain that their dwellings are liable to be thrown out of level by the rising or sinking of the tarry foundations.

Gardens and fruit trees flourish in the few inches of soil which cover the bitumen.

A road leads up from the landing to some sugar estates beyond the lake. It ascends a gentle slope of hardened pitch which where left to itself is covered with a dense growth of reeds and bushes, but where broken up by cultivation produces abundantly the usual tropical garden fruits.

In some places the bitumen is so mixed with earth as to lose some of its toughness as well as some shades of its pitchy hue.

Just above the village, in a small clearing, some twenty graves are marked by black crosses and still blacker mounds. They are excavated in the same sombre-tinted material. It would be interesting to know whether the bituminous matter has exerted any of its well known preservative powers upon the bodies thus entombed in it; but I could not learn that either accident or design had ever invaded any one of these strange sepulchres.

The road itself is a fine illustration of the adaptation of the pitch to the purposes of paving. Where too much mixed with earth it has become pulverized to the depth of a few inches. But in many places it is still so pure and solid that the wheels of heavily loaded sugar wagons and the hoofs of horses make but a slight and even that a transient impression.

In no part of the ascent from the shore of the lake does the stream of pitch appear to be covered by more than one or two feet of soil while in most places it is entirely bare. Its hardness is about that of gypsum. It breaks with a conchoidal but lusterless fracture. In places where the surface is not protected by vegetation it becomes so far softened by the sun as to be still making progress downward.

On nearing the lake the ascent becomes steeper, amounting to perhaps twenty-five feet in the last ten rods. Here the pitch is bare or but slightly covered with grass. Its appearance is not that of a sudden simultaneous overflow in a single smooth stream, but that of a great number of streams each but a few yards or rods in breadth.

These independent streams have jostled one another strangely in their progress. Their surfaces are wrinkled and drawn out into all manner of contortions, and where the edges meet, small ridges have been thrown up and the pitch broken into fragments not unlike the scoriæ of lava currents. These fragments of pitch were on fire in several places, having been kindled by a fire which ran through the "bush" a few weeks before. It is fortunate that the pitch when compact will not kindle, or in other words will not burn without a wick, for otherwise the entire region including the village of La Braye might suffer the fate of Sodom and Gomorrah.

The distance from the landing to the lake is three-fourths of a mile, the rise ninety-six feet.

The direction of the principal stream from the lake is due north.

On ascending the last slope of this pitchy glacier a singular scene meets the eye. A black and circular plain of pitch half a mile in diameter lies flush with the edge of the stream. It is surrounded by a dense wall of forest in which various species of tall palms are most conspicuous.

The lake itself is entirely bare of vegetation, except about twenty small clumps of trees which are arranged in a sort of broken circle about half way from the center to the circumference.

On a closer inspection the entire surface of this circular plain is seen to be intersected by a network of water channels Its appearance is exactly that of marbled paper. The pitch is divided into flat or slightly convex areas mostly polygonal but sometimes circular. They vary from one to eight rods in diameter. The intervening spaces are full of water. At the time of my visit, which was in the month of May and just at the close of the dry season, the water was so far reduced that it was easy to pass over all parts of the lake by leaping the channels. At other seasons this is more difficult.

These channels have heretofore been described as crevices or cracks in the pitch. This description is however incorrect for the material though apparently almost as hard as stone is yet far too plastic to admit of any thing like a fissure remaining open in it. Excavations from which many tons of pitch have been taken for exportation are closed up again in the course of a few days or weeks, not by streams of pitch flowing into them but by the gradual closing in of the sides and bottom.

The channels are produced and maintained by the following singular process. Each of the many hundred areas into which the lake is divided possesses an independent revolving motion in this wise. In the center of the area the pitch is constantly rising up —not breaking out in streams, but rising *en masse.* It is thus constantly displacing that which previously occupied the center and forcing it towards the circumference.

The surface becomes covered with concentric wrinkles, and the interior structure somewhat laminated, while the upper laminæ at the center of the area are torn into shreds by the expansion like the outer bark of a rapidly growing tree.

Where the edge of such an expanding area meets that of the adjoining one the pitch rolls under to be thrown up again in the center at some future period. The material is rarely soft enough to meet and form a close joint at the top but descends with a rounded edge and at a considerable angle.

The spaces thus left between the different areas are often five or six feet deep and three or four yards wide at the top, diminishing of course to a mere seam at the bottom.

Where three or more of them meet a star shaped cavity is formed in some cases twelve or fourteen feet deep.

It is difficult to conceive of a motion like this going on in a material almost of stony hardness, but that such a revolution is constantly taking place over the entire surface of this lake cannot be doubted.

Another curious proof of it is afforded by numerous pieces of wood which being involved in the pitch are constautly coming to the surface. They are often several feet in length and five or six inches in diameter. On reaching the surface they generally assume an upright position, one end being detained in the pitch while the other is elevated by the lifting of the middle. They may be seen at frequent intervals all over the lake standing up to the height of two or even three feet. They look like stumps of trees, protruding through the pitch, but their parvenu character is curiously betrayed by a ragged cap of pitch which invariably covers the top and hangs down like hounds ears on either side.

The conclusion then to which a close observation leads us in regard to the present condition of this singular lake is, not that it has suddenly cooled down from a boiling state as heretofore de-

scribed, but that, solid as the material is, it is still boiling although with an indefinitely slow motion. As the descent of the glaciers may be considered the slowest instance of flowing in nature so the revolutions of the scarcely less solid bitumen of this lake may be set down as the slowest example of ebullition.

The water which fills the crevices of the pitch is clear and very pure, especially towards the margin of the lake. It is the favorite resort of all the washerwomen for miles around.

So completely does the lake occupy the summit of the peninsula on which it is situated that water was observed flowing from the connected network level at eight nearly equidistant points of the circumference.

As the water is flowing now the pitch has formerly flowed from the lake in nearly all directions. It covers almost the whole peninsula like a mantle, reaching down to the sea and forming almost three miles of coast. The entire surface covered by it is estimated at three thousand acres. The lake itself contains one hundred acres. At the village of La Braye the stream of pitch has been dug through in several places, averaging from fifteen to eighteen feet in depth. Its depth at other places is not known.

Towards the center of the lake several detached areas are met with which are still quite soft. These have a glossy black surface. None of them are more than two or three rods in breadth. Those adjoining are rough and hard though not as hard as those nearer the margin.

The surface of these softer areas yields under the feet. On standing a few minutes one feels that he is gradually settling down, and in the course of ten or fifteen minutes he may find himself ancle deep.

In a few places indeed where streams of fluid pitch were oozing through the more indurated surface, a few minutes standing would sink one to the knees.

The pores of the pitch are full of water which oozes out on the slightest pressure, and by moistening the skin prevents adhesion.

Were it not for this the above experiment would not be altogether so safe or agreeable.

A heavy body placed on the pitch or a person standing long enough, would undoubtedly sink and perhaps disappear in it. But in no place was it possible to form those bowl-like depressions around the observer as described by former travellers. It may be that the material has become much harder since the first accounts of it were written; but it is difficult to understand how the weight of a man could ever have displaced a mass of pitch equal to a "great bowl" as deep as the shoulders, especially as the pitch is specifically much heavier than the human body.

The water in the vicinity of the soft pitch is colored dark green by carbonaceous matter. It has also a saltish taste and a disagreeable odor. A strong smell of bitumen is perceivable in the air.

Streams of gas issue from below, sometimes rising through the water but more frequently hissing and gurgling from small openings in the pitch above water level. It appears to be chiefly sulphuretted hydrogen, smelling strongly of that gas and instantly blackening a silver coin laid among the bubbles. When inflamed it burns with a pale yellowish flame.

The surface of the pitch is whitened in places by a deposit of sulphur.

The temperature of one of the streams of gas was 97° Fah., the highest heat which I observed upon the lake.

The water in some of the crevices was 95°. In such cases it appeared to be rising at one end of the opening, flowing along, and descending at the other.

I have no doubt that the mere surface of the pitch is sometimes heated by the sun to a higher degree than this. But the copious streams of gas would certainly indicate the fact if a much higher heat existed at a moderate depth below.

The pitch where most fluid had a temperature of only 95°.

It is evident that the bitumen does not owe its fluidity in any great degree to heat. It is true that the already hardened pitch may be melted by a sufficient heat, but that which is already fluid remains so at all ordinary temperatures.

Wherever it oozes out in streams it flows down over the hardened surface into the nearest channel of water (which may have a temperature not above 85°), where it creeps along the bottom in a stream that looks like a huge serpent.

The fluidity of the pitch is evidently owing to the oily matter which it contains. The whole thing seems more like a vast fountain of coal tar than anything else. The gradual hardening which has evidently taken place is due to oxydation and evaporation of the less fixed ingredients,—a process which the revolving motion heretofore described must greatly facilitate.

In one of the star shaped pools of water, some five feet deep, a column of pitch had been forced perpendicularly up from the bottom. On reaching the surface of the water it had expanded into a sort of center table about four feet in diameter but without touching the sides of the pool. The stem was about a foot in diameter. I leaped out upon this table and found that it not only sustained my weight but the elasticity of the stem enabled me to rock it from side to side. Pieces torn from the edge of this table sank readily showing that it had been raised by pressure and not by its buoyancy.

The vicinity of the pitch with its strong odor seems to give no more offense to animals than its presence in the soil does to vegetation. Numerous fishes ten or twelve inches long were seen in the water on the lake. An alligator shuffled off from one of the areas at my approach. In two instances I scared birds resembling

night hawks from their nests or rather from their eggs which were deposited upon the naked pitch.

In the course of several days spent in examining the lake and the region around it I walked several miles along the sea-shore both to the northward and to the southward of the lake. To the southward the shore is made up of bold cliffs upon which the sea is making rapid inroads. The strata consist of indurated clays of brilliant red and yellow colors. They present also thick veins of porcelain jasper.

Strata of loosely coherent sandstones also abound. These are more solid and durable where they are impregnated with bitumen which acts as a cement. Rounded pebbles of pitch and porcelain jasper form a beach at the foot of the cliffs.

About a mile and a half south of the lake I observed numerous beds of slightly indurated clay filled with the remains of leaves and vegetation. A little further on appears a bed of brown coal and lignite about twelve feet thick. It has such a dip and direction that if continuous it would pass under the lake at a great depth. But the strata are here much contorted and some even thrown into upright positions.

Pebbles of pure asphaltum are thrown up by the waves at this point and not far off the beach is blackened by brilliant titanic iron sand.

Nearer the lake and to the southwest of it, a large spring of petroleum breaks out under the sea. The escape of gases from this vent is sometimes so violent as to spout a column of water several feet high.

I passed over the place in a boat but at the time there was no ebullition although a strong odor of bitumen pervaded the sea breeze and acres of the sea were iridescent with the floating oil. The rocks on the beach opposite were varnished of a bright glossy black by the petroleum. I filled a bottle with it by skimming it up from the water with a palm leaf.

Many springs of petroleum occur in the interior, within a few miles of the lake. Two veins of pure asphaltum enclosed in clay were discovered about three-fourths of a mile from the same place. They were soon however exhausted, having yielded some twenty tons of the mineral sufficiently pure for varnishes.

From the point where the large spring of petroleum breaks out under the sea, at least three miles of the shore to the northward consists mainly of streams of pitch from the lake. There are a few intervals of sandstone and clays where elevations have diverted the bituminous currents. But all the most prominent head-lands are those which are defended by the indurated pitch. This material has in fact flowed out to a considerable, though as yet unknown, distance under the sea.

About a mile to the northward of the lake another bed of brown coal crops out upon the shore. It is about twenty feet

thick. Other and perhaps much thicker beds may exist in the vast mass of stratified materials which make up the bulk of the island.

From the occurrence of such considerable accumulations of vegetable matter so situated as apparently to pass under the lake, it seems reasonable to regard them as the source of the pitchy matter which rises in such quantity there.

Indeed many pieces of wood may be observed in the beds of brown coal which differ in no respect in their appearance from many of the pieces thrown up in the lake itself.

These beds of vegetable matter are probably undergoing a slow distillation by volcanic heat. It is true there are no evidences of volcanic eruptions in the vicinity of the lake nor any materials of volcanic origin scattered on the beach except perhaps the titaniferous iron sand. But at Cedras, twenty miles to the southward, there are active mud volcanoes. I did not have opportunity to examine them or to ascertain their character minutely. But the fact of their existence as well as the disturbed condition of the recent strata, together with the proximity of the island to the coast of Cumana where earthquakes are frequent and severe, appears sufficient to show that the island is not entirely free from volcanic action.

Various attempts have been made to apply the inexhaustible store of bitumen afforded by the lake, to some useful purpose. Mixed with sand and pebbles it is much used for pavements and the ground floors of houses at Port au Spain, a purpose for which it is admirably adapted.

It has been employed to advantage as fuel by the American steamers plying on the Orinoco. It is thrown in the furnaces among the wood, fusing too readily to be used alone.

With ten per cent of rosin oil it forms an excellent pitch for vessels.

The Earl of Dundonald has purchased a large tract of the pitch lands including twenty six acres of the lake and has instituted various experiments with the view of substituting the bitumen for India rubber and Gutta percha in the manufacture of water proof fabrics, covering of telegraph wires, &c. Judging from the specimens of water proof cloth, tubing and telegraph wire which were shown me by his agent at Port au Spain, (Mr. C. F. Stollmeyer,) these efforts bid fair to be quite successful.

It seems only necessary that the required amount of intelligent enterprise should be directed to the subject in order to render this wonderful reservoir of bitumen a source of great individual profit and of essential service to mankind.

Art. XIV.—*The Harrison Tornado, Ohio, February* 14, 1854.

It is of interest in illustration of the subject of storms, to notice briefly the Harrison tornado, as its action differs in some respects from the Brandon storm. Many of its operations were chronicled as being of such a wonderful and unheard of character, that the writer was induced to visit the scenes of its ravages, to examine and record the facts, of whatever nature they might be.

During no season since, perhaps, the settlement of the state, have so many visitations of the kind occurred; and the opportunity afforded has been uncommonly favorable for investigating the phenomena of these storms.

The tornado under consideration, commenced, as far as can be learned, in Dearborn Co., Ia., 10 or 12 miles west of Harrison, in North Latitude 39° 10′ and West Longitude 70°. Its course was N. 72° 32′ E., ravaging the country, at intervals for 50 miles.

Its most marked characteristic was a diffuse and feeble action at the circumference, and intense energy at the axis. Occasionally it struck objects along its axis with the spite of a fury; closely approximating in its sudden and destructive effects, to the blast from a huge cannon. The most striking examples of its violence were found at the Graham place.

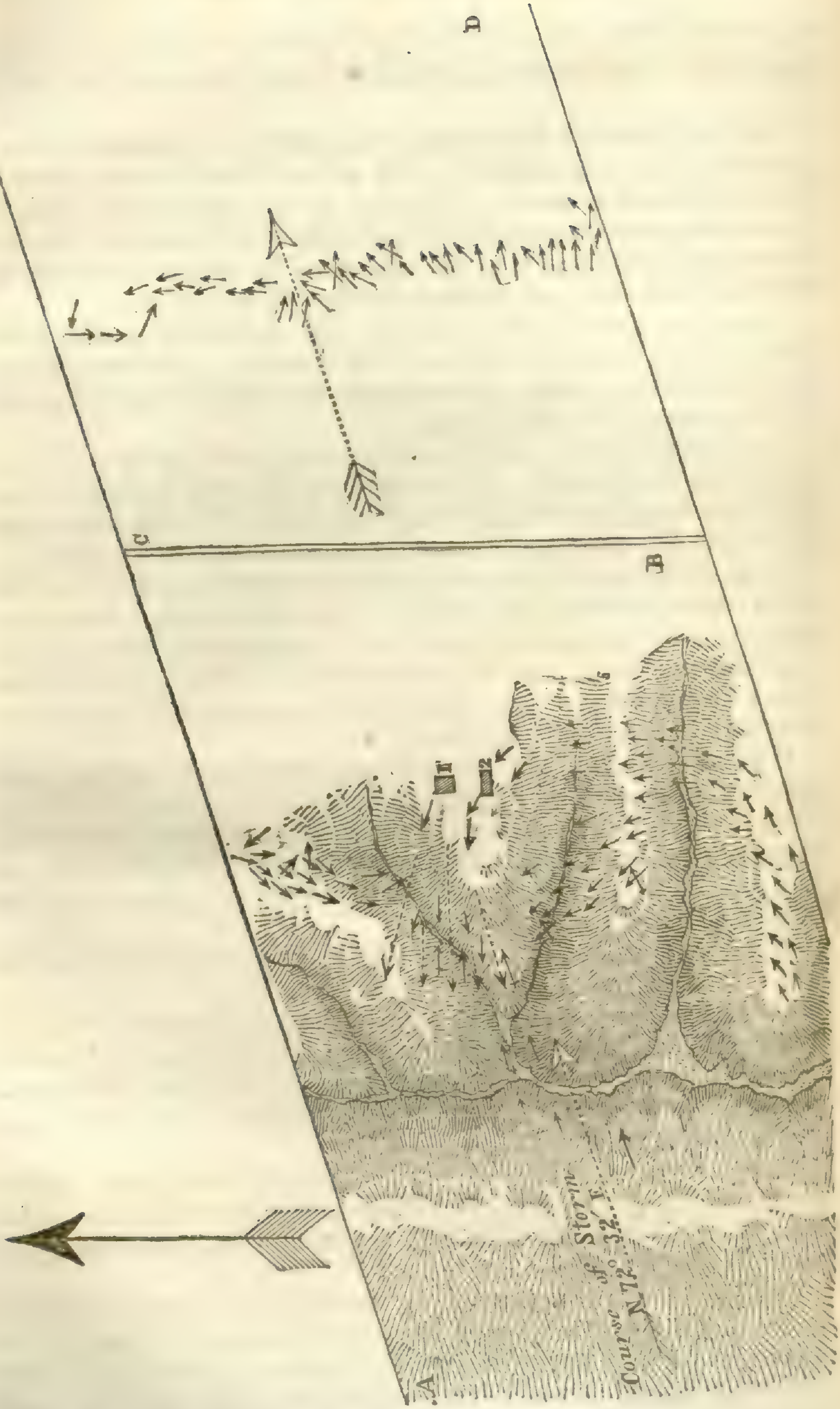
West of Harrison the country was very much broken; and generally wherever the tornado ascended a hill or crossed its top, it left the forest untouched; but the moment it began its descent, everything fell before it. It frequently rose from the earth, or was so broken up by the obstacles encountered, that extensive tracts were passed without injury.

Its destructive effects were mainly confined to a path varying from 200 to 600 feet in breadth.

At the Graham place it was three-fourths of a mile broad; at Mr. Wakefield's woods 66 rods; and at Dr. Bowles, from the extreme point on the right, where fences in the open fields were prostrated, to the left where trees were thrown down, must have been a little more than half a mile.

Careful observations were made at the Graham place, by the writer assisted by the Rev. M. Golliday of Harrison; also at the latter place; and at Mr. Wakefields; and a somewhat hasty examination at Dr. Bowles.

The Graham house (I) stood in the edge of a forest which extended to the west, with open ground on the east. West of the house (see plot) about ¼th of a mile, a narrow and deep valley runs north and south. Lateral valleys intersect this nearly at right angles from the east. These are also deep and narrow gorges, extending about one-fourth of a mile back from the main valley, forming sharp and well defined ridges between them. At the



A B, Topography of the Graham place, and plot of the tornado: Breadth $\frac{3}{4}$ mile.
C D, Plot of survey across Wakefield's woods: Breadth 66 rods.

head of one of these valleys and just where the descent into it begins, stood a small frame house. The action of the wind upon the house and the forest around, was extremely complex; and while passing across the track with a compass, the whole effect seemed like a riddle designed to puzzle meteorologists. On this account the survey was minute and protracted. The path was divided into sections of three rods each. The plot gives but a very small part of the bearings which were taken; in some cases a single arrow on the plot represents the mean of from three to ten bearings. The topography of the place is also represented as the complex action referred to, is believed to have been owing to the nature of the surface. The arrows give the directions of the currents which simultaneously or in succession swept over this ill-fated place.

The storm approached the house nearly from the west; and yet it was struck on the eastern side, by the current from the south-east. The proof of this is explicit. The whirling mass of vapor, attended with a thundering roar, was seen by Mr. Graham approaching from the west: he shut the door, a few moments of awful suspense followed: then a window over the east door was driven in with a loud report; then the door followed; and the next instant the house was torn from its foundations and shivered to fragments.

The tenants bruised and bleeding, and with tattered garments, were found in different positions.

The dotted arrows indicate the directions in which the fragments were carried. They were strewed over the ground for about 30 rods west of the house, and lay between N. W. and W. 6° S. A part of the roof was carried W. 6° S.; the chimney thrown W. 11° N.; a piece of a stove was picked up 30 rods N. W. Along these same bearings were portions of chairs, tables, bedsteads, boxes, boards, timbers, &c. It is a singular fact and shows the violence of the wind, that scarcely an article or a timber could be found in an entire state.

The barn (2) which was built of logs, fell to the N. W.; several trees near it in the same direction.

West of the house large trees lay between W. and W. 30° N. The bearings and positions of these are shown by arrows. Mingled with these are arrows with cross bars pointing easterly. These represent trees with their limbs lying above the other trees which had been thrown towards the west; and in a few instances fragments of the house were found beneath them.

This proves that the wind which first swept over the house was from the east, and afterwards it came with equal violence from the west. The latter action; that from the west, was doubtless caused by the passage of the axis of the storm.

One remarkable feature of the storm at this point, which will strike the meteorologist upon a bare inspection of the plot, is, that the current which destroyed the house should have been so far in advance of the axis as to carry fragments 30 rods to the west, before it curved into and mingled with the axis.

A second feature is that such unparalleled violence in the overthrow of the house, should have been exhibited by the outskirts of the storm. When the house fell, the axis could not have been less than 40 rods from it. Usually in narrow tornados, a point 40 rods from the axis is out of the range of violence.

What effect the contour of the ground may have had in producing these results, I leave meteorologists to judge. The portion of the tornado which struck the house, can be easily traced up the valley, which lies near the right hand border of the storms path. It passed up the valley and when near the head, plunged down the slope, and bridging over the bottom with fallen trees, ascended the opposite side, and gradually curved round into the axis. Portions of the house which were first swept around westward into the axis, were subsequently carried by it towards the east. A tea spoon was found E. 30° S. from the house, half a mile distant. A part of an eve-trough near the same place.

A piece of a chest easily identified, was carried N. E. two miles, and fell a mile north of the track of the storm. If the tornado had the form of an inverted cone, like the miniature whirls so often seen, then the transportation of this stick upwards and outwards is easily explained. Other explanations might indeed be given; but they are scarcely necessary unless the rotary action be denied.

Five miles east of the Graham place, the tornado descended a high and steep range of hills which enclose the Whitewater river on the west, prostrating as usual the forest in its descent.

In crossing the valley, it passed through the south part of Harrison. Fortunately the track of destructive violence was here narrowed to two or three hundred feet

The roof of a large church was raised perpendicularly into the air, and carried over the steeple towards the east. A tin ball two feet in diameter on the top of the spire was nearly torn from the iron rod which passed through it. There was no bruise upon it to indicate that it had been struck by a hard body.

The roof of a carpenter's shop was taken off and the posts broken just below the upper story. The lower story was thrown down, and the upper fell back upon the fragments without further injury. Two workmen were in the upper part, and were not aware of their descent till the door, which led to a flight of steps on the outside of the building was opened.

It was supposed by some intelligent observers that the effects produced upon the shop and some other buildings were indicative

of a violent explosive action from within. With one exception I found no evidence of anything more than the usual ascending current in the axis of tornadoes. The windows of the shop were driven in before the roof went off: the gable end of the church fell inside.

Where both walls gave way, one as far as I could ascertain fell in and the other out.

The wall of a small house was sprung out on the north, so that the ceiling was detached. A lady who was in the house at the time stated that the south door was first driven in with great violence.

The exception referred to was a brick house: A portion of the south wall fell outwards, but whether a current of air had found its way to the inside through a door or window could not be ascertained.

Five miles east of Harrison the tornado struck a dense forest. On account of the level surface and the open country around, the position was deemed a favorable one for giving a truthful record of the mode of the storms action. Here as at the Graham place, the track was divided into sections of three rods each, and the bearings of the fallen trees taken with a compass. These bearings are carefully laid down in the plot. The original division of this part of the State into sections of a mile square by the United States, afforded a means of determining accurately the course of the storm.

In crossing five sections, the tornado diverged to the north one and a half miles.

The following table gives the bearings of the last survey; and is inserted for the benefit of those who may be desirous of examining critically the law of storms. The order of the bearings is the same as in the tables belonging to the Brandon storm,—from right to left across the track.

SURVEY ACROSS WAKEFIELD WOODS.

Whole breadth, 66 rods.		Bearings.	Whole breadth, 66 rods.		Bearings.
1st section of 3 rods,	1	E. 5° S.	7th section of 3 rods,	14	N. 77° E.
	2	E. 5° S.		15	N. 45° E.
	3	E.		16	N. 75° E.
	4	N. 45° E.		17	N. 45° E.
2nd "	5	N. 52° E.	8th "	18	N. 60° E.
	6	E.		19	N. 50° E.
3d "	7	E.		20	N. 30° E.
	8	E.		21	N. 55° E.
4th "	9	E.	9th "	22	E. across 20
	10	N. 45° E.		23	N. 45° E.
5th "	11	E.		24	N. 50° E.
6th "	12	N. 85° E.	10th "	25	N. 30° E.
	13	N. 78° E.			

TABLE—*Continued.*

Whole breadth, 66 rods.		Bearings.
11th section of 3 rods,	26	N. 55° E.
	27	N. 50° E.
	28	E. over 27
	29	N.
	30	N. 20° E.
	31	N. 45° E.
12th " "	32	E. twisted
	33	E. 5° S. do.
13th " "	34	E. 10° S.
	35	N.
	36	N. 10° E.
14th " "	37	N. 15° E.
	38	N. 5° W.
15th section of 3 rods,	39	N.
	40	N. 20° W.
	41	N. 6° E.
16th " "	42	N. 6° E.
	43	N. 6° E.
	44	S. 68° E.
17th " "	0	0
18th " "	45	N. 20° W.
19th " "	0	0
20th " "	46	N. 68° W.
21st " "	47	S. 10° E.
22d " "	48	S.

The progressive velocity of the tornado could not be ascertained accurately. It was about ten minutes in passing from Harrison to New Haven, a distance of five miles. This would give thirty miles per hour. This estimate cannot be far from the truth; from some circumstances I am inclined to believe that it is a little too high.

A few general remarks will close this article.

1st. This storm was remarkable for the occasional exhibitions of extreme violence. The Graham place afforded the most striking examples. The house was not merely thrown down, but shivered. A tea spoon was carried half a mile; and a piece of a stove thirty rods. A heavy waggon was carried a few rods and dashed to fragments against a tree. A piece of scantling twelve feet long and three by four inches, was taken thirty rods east, and driven into the ground *three and a half feet.* Four men tried in vain to pull it out; and the writer in connection with Mr. Graham, succeeded only after digging around it to the depth of nearly three feet. The earth thus penetrated was, with the exception of the first six inches, a stiff, yellow clay. What renders this incident more striking, was the fact that the end of the scantling was not pointed. The cross section presented a resisting surface of eight square inches, and the whole amount of earth displaced was equal to 336 cubic inches. According to experiments made at the New York Navy Yard, a shot 6·8 inches in diameter would penetrate earth five feet nine inches. A shot of three inches in diameter, nearly the resisting surface of the scantling, would under similar circumstances penetrate three feet nine inches. This is but three inches deeper than the scantling was driven. While the weight of the timber in question would have been greater than the weight of a three inch shot; on the other hand the form of the end was not so favorable for penetration. What portion of its velocity was due to falling cannot be told with accuracy. As it fell within thirty rods of the building from

which it was taken, it could not have ascended to a great beight. It entered the ground at an angle of about 45° ; and if from this we estimate the velocity acquired from falling, to equal one-half of the whole there is still left a velocity of 500 feet per second due to the wind. The effects of such a wind would be fearful indeed : it would move at the rate of 340 miles per hour.

2nd. The involute form of the curve described by the wind as it approached the axis appears more marked in this than in the Brandon storm. The plots of both Surveys indicate this, more especially that at the Graham place. At the same time the cycloidal curve is easily made out. The reverse action of the loop is clearly exhibited at the Graham place ; the direct and reverse at Wakefield's. Most of the prostrations at Wakefield's on the right of the axis, were not made by the front of the storm, but took place when about one-half of the cycloid had passed over. Some of the trees were turned outward, showing that they stood till struck by the heel of the storm.

3d. The plunging action of the tornado was a remarkable feature. The ground west of the Whitewater was favorable for observing this, as it was intersected by deep ravines. Ascending slopes were touched lightly in general, or not at all ; while the descending were often swept with fearful violence. I am not aware that this peculiarity has been mentioned by other observers. The explanation does not seem difficult ; but too much space has already been occupied to state it here.

In conclusion the writer would take the liberty to suggest to observers, that he has found it important to carry his observations beyond the track of greatest violence. Though no trees nor houses may be thrown down, yet valuable evidence to show the mode of action can oftentimes be obtained.

Mr. Laird's house in the vicinity of the Graham place, was on the left of the axis, but too far from it to suffer any injury. The wind was violent but left none of the ordinary marks which could determine its direction. Mr. Laird however stated to the writer that the wind first blew in the south door, and two men were unable to shut it. A moment afterwards, and the north door which had been locked, was violently driven in. The direct and reverse stroke of the loop seem pointed out here. The action of a tornado along the axis only, affords but confused data to elucidate the laws which govern it.

Art. XV.—*On the Geographical Distribution of Crustacea;* by James D. Dana.

(Continued from vols. xvi, xviii, and xix.)

Having in the preceding pages on the geographical distribution of Crustacea, treated of their distribution according to zones of temperature, I now take up the other branch of the subject.—

The Distribution of Crustacea according to Geographical Provinces.

In presenting a series of tables in which the distribution of the Genera is given, I divide the surface of the globe, for marine zoological geography, into three sections, the *Occidental*, the *Africo-European*, and the *Oriental;* the first, including the east and west coasts of America and adjoining islands; the second, the eastern side of the Atlantic Ocean, the coasts of Europe, and also of Africa as far as the Cape of Good Hope; the third, embracing the Indian Ocean, and its coasts and islands, the East Indies and the Pacific Ocean, with its coasts and islands, exclusive of the western coast of America and the neighboring islands. The total number of species in each is given in a separate column.

I make further groupings or subdivisions, by which the several portions of these great regions are distinguished. These general tables are not here copied from the author's Report, and particular explanations therefore need not be given.

The following is an abstract of some of the results:

The division A, includes the Atlantic and Pacific coasts and islands of America; B, the European and West African coasts and islands, from Cape Horn to Greenland inclusive; and C, the coasts and islands of the Indian and Pacific Oceans (America excluded).*

I. BRACHYURA.

	A.	B.	C.
Maioidea.			
Maiinea,	69	24 (1 *a*)†	63 (1 *b*)†
Parthenopinea,	1	5	29
Oncininea,	0	0	2
Total Maioidea,	70	29 (1)	104 (1)
Cancroidea,			
Cancridæ,	10	3	1
Xanthidæ	17	7 (1 *a*)	129 (1 *b*)
Eriphidæ,	7	5	52 (1 *b*)
Fortunidæ, Platyonychidæ and Podophthalmidæ,	13	19 (1 *a*)	54 (1 *a*)
Telphusinea,	6	1	7
Cyclinea,	1	0	0
Total Cancroidea,	54	35 (2)	243 (3)

* The discrepancies between the enumeration here and the summaries of the preceding tables, arise from species omitted in one or both, on account of the uncertainty of their localities.

† 1*a*, means that 1 of the 24 is identical with a species under A; and 1*b*, that 1 of the 63, is identical with a species under B. So, below.

Grapsoidea,	51	18 (5)	124 (5 *a*)
Leucosoidea,	9	12	48 (1 *b*)
Corystoidea,	6	5	8
Total Brachyura,	190	90 (8)	526 (10)

II. ANOMOURA.

	A.	B.	C.
Dromidea,	1	9	15 (1 *b*)
Bellidea,	2	0	0
Raninidea,	1	0	5
Hippidea,	7	2	7
Porcellanidea,	24	4	19
Lithodea.	5	1 (1 *a*)	3
Paguridea,	26	27 (1 *a*)	61 (1 *b*)
Ægleidea,	2	0	0
Galatheidea,	3	6 (1 *a*)	5
Total Anomoura,	71	49 (3)	115 (2)

III. MACROURA.

	A.	B.	C.
Thalassinidea,	7	8	9 (1 *b*)
Astacidea,	29	9	27
Caridea,	40	77 (3 *a*)	85 (3 *b*)
Penæidea,	4	8	22
Total Macroura,	80	102 (3)	143 (4)

IV. ANOMOBRANCHIATA.

	A.	B.	C.
Squilloidea,	10	16	32 (3 *b*)
Mysidea,	3	18	16
Amphionidea	0	9	11
Total Anomobranchiata,	13	43	58 (3)

V. TETRADECAPODA.

	A.	B.	C.
Isopoda.			
Idoteidea,	11	25	6 (1 *b*)
Oniscoidea,	30	72 (1 *a*)	11
Cymothoidea,	32	57 (1 *a*)	42 (2 *b*)
Total Isopoda,	73	154 (2)	59 (3)
Anisopoda,	10	38	6
Amphipoda.			
Caprellidea,	13	24	6
Gammaridea,	55	114	51
Hyperidea,	9	27	17
Total Amphipoda,	77	165	74
Total Tetradecapoda,	160	357 (2)	139 (3)

The table affords the following lists of genera of the three grand divisions, according to the present state of the science.

1. GENERA EXCLUSIVELY AMERICAN OR OCCIDENTAL.

	Coast on which found.
1. *Maioidea.*	
Microrhynchus,	west.
Salacia,	"
Libidoclea,	west and east.
Libinia,	" "
Pelia,	west.
Rhodia,	"
Pisoides,	"
Thoe,	west and east.
Chorilia,	west.
Scyra,	"
Othonia,	"
Mithraculus,	west and east.
Tyche,	" "
Eurypodius,	" "
Oregonia,	west.
Inachoides,	"
Pugettia,	"
Epialtus,	west and east.
Leucippa,	" "
2. *Cancroidea.*	
Pilumnoides,	west.
Trichodactylus,	east.
Arenæus,	"
Potamia,	west and east.
Orthostoma,	east.
Acanthocyclus,	west.
3. *Grapsoidea.*	
Cyrtograpsus,	east.
Uca,	west and east.
Gecarcoidea,	east.
Fabia,	west.
Pinnixa,	west and east.
Pinnotherelia,	west.
Halicarcinus,	west and east.
4. *Leucosoidea.*	
Platymera,	west.
Hepatus,	west and east.
Guaia,	" "
5. *Corystoidea.*	
Telmessus,	west.
Peltarion,	east.
Pseudocorystes,	west.
6. *Anomoura.*	
Corystoides,	west.
Bellia,	"
Ranilia,	"
Albunhippa,	west.
Echidnocerus,	"
Macroura.	
7. Cambarus,	west and east.
Paracrangon,	west.
Æglea,	"
Cryphiops,	"

2. GENERA EXCLUSIVELY OF THE AFRICO-EUROPEAN DIVISION.

1. *Maioidea.*—Lissa, Stenorhynchus, Amathia, Eurynome.
2. *Cancroidea.*—Perimela, Portumnus, Polybius.
3. *Grapsoidea.*—Gonoplax, Heterograpsus, Brachynotus, Hymenosoma.
4. *Leucosoidea.*—Ilia.
5. *Corystoidea.*—Thia, Corystes.
6. *Anomoura.*—Homola.
7. *Macroura.*—Axius, Calocaris, Ephyra, Gnathophyllum.

3. GENERA EXCLUSIVELY ORIENTAL, OR OF THE THIRD DIVISION.

1. *Maioidea.*—Macrocheira, Paramithrax, Micippa, Lahaina, Naxia, Hyastenus, Pyria, Cyclax, Camposcia, Paramicippa, Tiarinia, Perinea, Halimus, Menæthius, Stenocionops, Huenia, Xenocarcinus, Parthenope, Eumedonus, Ceratocarcinus, Gonatonotus, Eurynolambrus.

2. *Cancroidea.*—Atergatis, Liomera, Liagora, Medæus, Halimede, Etisus, Carpilodes, Zozymus, Daïra, Cymo, Polydectus, Œthra, Galene, Pseudozius, Melia, Acanthodes, Actumnus, Ruppellia, Domæcius, Trapezia, Tetralia, Quadrella, Scylla, Charybdis, Lissocarcinus, Podophthalmus.

3. *Grapsoidea.*—Curtonotus, Cleistostoma, Macrophthalmus, Helœcius, Scopimera, Doto, Eriocheir, Platynotus, Trichopus, Sarmatium, Helice, Gecarcinicus, Xenophthalmus, Xanthasia, Hymenicus, Elamena, Myctiris.

4. *Leucosoidea.*—Mursia, Orythia, Thealia, Matuta, Philyra, Leucisca, Nucia, Nursia, Myra, Ixa, Iphis, Arcania, Oreophorus, Tlos, Ethusa.

5. *Corystoidea.*—Kraussia, Œidia, Dicera.
6. *Anomoura.*—Caphyra, Raninoides, Ranina, Notopus, Lyreidus, Cosmonotus, Lomis, Diogenes, Aniculus, Birgus.
7. *Macroura.*—Laomedia, Glaucothoe, Callianidea, Callisea, Thenus, Ibacus, Astacoides, Paranephrops, Cyclorhynchus, Atyoida, Alope, Œdipus, Harpilius, Anchistia, Palæmonella, Hymenocera, Oplophorus, Regulus, Stenopus, Spongicola, Acetes, Eucopia.

4. GENERA COMMON TO THE AMERICAN AND AFRICO-EUROPEAN DIVISIONS, BUT NOT IN THE THIRD, OR ORIENTAL.

1. *Maioidea.*—Hyas, Herbstia, Leptopodia, Stenorhynchus.
2. *Cancroidea.*—Atelecyclus.
3. *Anomoura.*—Munida, Grimothea.
4. *Macroura.*—Homarus.

5. GENERA COMMON TO THE AFRICO-EUROPEAN AND ORIENTAL DIVISIONS, NOT YET FOUND IN THE OCCIDENTAL.

1. *Maioidea.*—Inachus, Doclea, Maia, Achæus, Lambrus.
2. *Cancroidea.*—Actæa, Actæodes, Thalamita, Portunus, Telphusa.
3. *Leucosoidea.*—Cycloes, Ebalia, Dorippe.
4. *Anomoura.*—Latreillia, Cymopolia, Remipes.
5. *Macroura.*—Nika, Lysmata, Caridinia.

6. GENERA COMMON TO THE THREE DIVISIONS.

1. *Maioidea.*—Pisa, Mithrax (mainly Occid.), Acanthonyx.
2. *Cancroidea.*—Xantho, Panopæus (mainly Occidental), Pilumnus, Eriphia, Lupa, Amphitrite, Carcinus, Platyonychus.
3. *Grapsoidea.*—Grapsus, Goniograpsus, Sesarma (sparingly European), Acanthopus, Plagusia, Pinnothera, Calappa.
4. *Anomoura.*—Dromia (sparingly Occid.), Albunæa, Porcellana, Lithodes, Paguristes, Bernhardus, Pagurus (mainly Orient.), Clibanarius, Galathea.
5. *Macroura.*—Gebia, Scyllarus, Panulirus, Palinurus, Astacus, Crangon, Alpheus, Betæus, Hippolyte, Pandalus, Palæmon, Sicyonia, Penæus.

The following are lists of *species* common to two or more of the three divisions. They may be much changed by farther study, through the discovery that the specimens from distant localities are not conspecific. Should this happen, there is a relation indicated based on their close similarity, which is important.

1. SPECIES STATED TO BE COMMON TO DIVISIONS A. AND B., OR THE AMERICAN AND THE AFRICO-EUROPEAN WATERS.

Hyas coarctata: Massachusetts and Long Island, in United States; France; England; Shetlands.

Leptopodia sagittaria: Canaries; West Indies; Valparaiso.

Panopæus Herbstii: Mediterranean; Key West, South Carolina, and New York in United States.

Carcinus mænas : Mediterranean at Nice ; Crimea ; England ; Massachusetts, United States.

Grapsus pictus : Madeira ; Peru and Chili ; (also various Pacific islands).

Planes minutus : Atlantic Ocean, and occasionally found on both the American and European coasts.

Goniograpsus varius : Canaries ; Mediterranean at Algiers, Nice, Italy ; Crimea ; Brittany ; and probably at Rio Janeiro, Brazil.

Sesarma reticulata : Key West and South Carolina, in United States ; and in South Africa, according to McLeay.

Acanthopus planissimus : West Indies ; Canaries ; Madeira ; Cape Town and Port Natal, South Africa ; (also various tropical Pacific islands).

Plagusia squamosa : West Indies ; Key West, South Carolina, in United States ; Canaries ; Madeira (also, Isle of France ; Indian Ocean ; Red Sea ; Port Natal).

Plagusia tomentosa : Chili ; Cape Town ; (also New Zealand).

Albunæa symnista : Canaries ; Mediterranean (also Pondicherry) ; and if the *A. oxyophthalmus* is the same species, it occurs in the West Indies, and on the coast of South Carolina.

Lithodes Maia : Great Britain ; Shetlands ; Norway ; coast of Massachusetts (rare).

Bernhardus streblonyx : Great Britain ; France ; Mediterranean ; Norway ; Massachusetts, in United States ; (also Kamtschatka).

Cenobita diogenes : West Indies ; Mediterranean, (Hawaii ?).

Crangon vulgaris : Great Britain ; France ; United States ; San Francisco and Puget's Sound, Western America.

Crangon boreas : Norway ; Iceland ; Greenland ; Massachusetts (in fish) ; (also, Kamtschatka).

Pandalus annulicornis : Scotland and Shetlands ; Norway ; Iceland ; Massachusetts (rare).

Gonodactylus chiragrus : Mediterranean ; Key West ; (also, Red Sea ; Port Natal, South Africa ; Isle of France ; East Indies ; Swan River, Australia ; Pacific Ocean, at Feejees, Tongatabu, &c.).

2. SPECIES COMMON TO B. AND C., THE AFRICO-EUROPEAN AND ORIENTAL SEAS.

Mithrax dichotomus : Mediterranean ; East Indies.

Achæus Cranchii : Mediterranean ; *Japan* (probably same species).

Actæa rufo-punctata : Canaries and Mediterranean ; Isle of France, Indian Ocean.

Thalamita admete : Canaries ; Port Natal, South Africa ; Red Sea ; Indian Ocean, and East Indies ; Pacific Ocean, at the Fejees, Samoa, Hawaiian Islands, Wake's Island, &c.

Pilumnus Forskalii : Canaries ; Red Sea.

Grapsus pictus : see above.

Grapsus strigosus : Canaries : South Africa ; Red Sea ; East Indies.

Goniograpsus messor : Canaries ; Port Natal, South Africa ; Red Sea ; East Indies.

Planes minutus : Atlantic ; *Japan.*

Acanthopus planissimus : see above.

Plagusia tomentosa: Chili; South Africa; New Zealand.
Plagusia squamosa: see above.
Cycloes granulosa: Canaries; *Japan* (probably same species).
Remipes scutellata: Ascension Island; Swan River, Australia; St. Christopher's.
Lysmata seticaudata: Mediterranean; *Japan.*
Alpheus Edwardsii: Mediterranean; Cape Verdes; Port Natal, South Africa.
Pandalus pristis: Mediterranean; *Japan.*
Squilla mantis: Mediterranean; Canaries; *Tschusan.*
Pagurus striatus: Mediterranean; Japan.

3. COSMOPOLITES.

The above lists include the following species occurring in the Occidental, Africo-European, and Oriental seas.

Grapsus pictus.
Acanthopus planissimus.
Plagusia sqamosa.
Plagusia tomentosa.
Bernhardus streblonyx.
Crangon boreas.
Crangon vulgaris.
Gonodactylus chiragrus.

These are cosmopolite species.* The *Grapsus*, *Acanthopus*, *Plagusia*, *squamosa* and *Gonodactylus* preëminently deserve this name, being found both north and scuth of the equator. They thrive in the hottest equatorial waters, and have their extreme limit in the temperate region. The temperature they admit of is hence at least from 56° to 88° F.

The other species are cold-water species. *Plagusia tomentosa* belongs to the southern subtemperate region, being reported from Cape Town, New Zealand, and Chili, and the rest are found in high northern latitudes, and probably pass from the Atlantic to the Pacific Oeean through the Arctic Seas.

Besides the above species, a few are found in the West Indies, which occur also in the Oriental Seas, but are not yet known from the European or West African coasts. These, which also may be styled cosmopolites, are as follows:

Mithrax asper: East Indies; probably the same on the Peruvian coasts.
Atergatis lobatus: Red Sea and Indian Ocean; West Indies.
Carpilius maculatus: East Indies; South France; Japan; various Pacific Islands from the Paumotus to the Feejees and Hawaiian Islands; West Indies; Eriphia gonagra; East Indies; Port Natal; Key West.
Menippe Rumphii: East Indies; Rio Janeiro and the West Indies.
Chlorodius exaratus: Pacific Islands; East Indies; West Indies.
Lysiosquilla scabricaudis: Indian Ocean; West Indies; Brazil; South Carolina.

* The *Platyonychus bipustulatus* may possibly be another cosmopolite, for it is reported from Table Bay, the East Indies, Japan and Valparaiso. But we believe the Valparaiso species to be different from that of the East Indies, and have so named it.

From the survey already made, it is apparent, that the three grand divisions of the seas and coasts adopted in the preceding table, have very few species in common, and they correspond to a natural geographical arrangement. They constitute three kingdoms, to which two should be added, one for the Arctic Seas, and the other for the Antarctic. These kingdoms are:

I. The *Occidental* Kingdom, embracing the Atlantic and Pacific coasts of America to the frigid region, or some point in the subfrigid region.

II. The *European* Kingdom, extending from Cape Horn (or Cape Agulhas) to the Shetlands inclusive, and embracing the adjoining islands.

III. The *Oriental* Kingdom, including the east coast of Africa, the south and east of Asia, and the islands of the Indian and Pacific Ocean, exclusive of the American continent.

IV. The *Arctic* Kingdom, including Norway, Iceland, Greenland, the Alascha Archipelago, and adjoining parts of the coasts of America und Kamtschatka, with other Arctic lands.

V. The *Antarctic* Kingdom, embracing Fuegia, the Falklands, Southern New Zealand, and the lands or islands of the Antarctic Seas.

It will not be understood that the torrid species in one of these kingdoms resembles the temperate more than do the torrid of another kingdom; for this is far from true, since the distribution of genera is to a great extent determined by temperature, as already shown. But taking the range of species of the kingdoms through, there is a striking difference between the kingdoms in species of the same temperature region or zone.

Each of the first three kingdoms are naturally divided into three subkingdoms: a *north*, a *middle*, and a *south*, corresponding severally to the North Temperate, Torrid, and South Temperate zones of sea temperature. The importance of these divisions will be a subject of further remark beyond.

The summary of the results in the preceding table, presents some striking facts.

We observe, first, that there is a ratio of 1 : 1·5 between the Maioids of the A and C divisions (that is between those of the *Occidental* and *Oriental* seas, as just explained), while the ratio is about 1 : 4½ for the Cancroids. So also, while the ratio of the A and B divisions together (Occidental and European) to C (Oriental) is for the Maioids, nearly 1 : 1, it is for the Cancroids, 1 : 3. Here is a wide difference between the Occidental and Oriental seas as regards these groups. This last ratio is for the Corystoids nearly that for the Maioids, or more exactly, 1 : 0·75; and for the Grapsoids it is 1 : 2; for the Leucosoids, 1 : 2½. (The Arctic and Antarctic Seas are here merged in the other kingdoms, with which they are most nearly associated.)

If we compare these ratios with those which the same groups sustain as regards temperature, as exhibited on a former page, we discover that there is a very close parallelism; showing plainly that the prevalence of Maioids in the Occidental Seas must be owing to the comparative prevalence of cold waters; and the prevalence of the warm water groups, the Cancroids and Leucosoids, in the Oriental Seas, is owing conversely to the great extent of warm waters.

Again, the ratio between the A and B divisions together of the Macroura, and the C division, is nearly as 1 : 0·8, which sustains the same conclusion as to the influence of temperature.

The corresponding ratio for the Tetradecapoda is as 1 : 0·26. But as this group, owing to the smallness of the species, has not been thoroughly investigated, except in European regions, directly under the eyes of European observers, we cannot use satisfactorily the facts they present for deducing general conclusions, or for characterizing zoological districts or provinces. Still, it should be observed that the facts conform to the same principle.

It is hence of the highest importance before comparing the zoological character of different coasts, that the temperature-regions of those coasts should be ascertained.

Comparative tables of the East Indies and Mediterranean, or of the Peruvian coast and the East Indies, or of the southeast and southwest coast of Africa (and so on), would lead us far astray, if this element were left out of view; for a difference of temperature region, implies a difference of genera and species, independent of other considerations. On these grounds, whole continents, or sides of continents, may have a common character and differ widely from other continents in the same latitude.

If we look at the American continent in this point of view, we at once perceive a striking peculiarity. All the coasts of North and South America with the Gallapagos on the west belong to the Temperate zone, excepting a few degrees along by Panama, and a connected range of coast from Key West to Rio Janeiro. Chili and Peru are excluded even from the warm temperate region, and so also, the coast of the United States, north of Cape Hatteras. (See Chart, this Jour., vol. xvi.)

Now contrast America with the Oriental Seas. The whole east coast of Africa, north of the parallel of 30° south, the coasts of India and the East India Islands, and the northern half of Australia, together with the numerous islands of the Pacific, belong alike to the Torrid zone. In the American Seas, the torrid coasts make a single range, and have many species in common throughout. In the Oriental Seas, they reach with an uninterrupted surface over one-half of the circumference of the globe, and there is room for many distinct provinces within the same temperature region. The fact is more striking, if we consider that the At-

lantic east of the West Indies contains no islands in the Torrid zone, besides St. Helena, Ascension, and the Cape Verdes, all of which are of small size.

Again in order to compare the coasts of America and Europe, we must observe that the warm temperate region is represented along the former by a small district from Northern Florida to Cape Hatteras, while this region does not reach at all the latter, and only the Canaries in the Eastern Atlantic are within it. Moreover, the temperate and subtemperate regions have no existence on the North American coast at Cape Hatteras; while on the European side, the former embraces the larger part of the Mediterranean, and a portion of North-western Africa, and the latter includes the Atlantic coast of Portugal. But north of Cape Hatteras, the coast of America is rightly compared with that of Europe, north of Portugal.

To compare the coast of Asia and Europe, we first observe in the same manner the temperature regions. There is in fact a striking similarity with the coast of the United States. Yet, the torrid and subtorrid regions are confined to limits much nearer the equator; and the warm temperate, although embracing as many degrees of latitude as the warm temperate on the United States, does not, on the China coasts extend farther north than the subtorrid region of the Florida coast. The temperate region hardly has a place on the coast of China, while the subtemperate occupies the Yellow Sea. North of this Gulf, the coast corresponds mostly with the coast of the United States, north of Cape Cod.

It is unnecessary to adduce other explanations, as the chart furnishes all that is needed for a ready comparison between the different coasts.

The propriety of uniting in one kingdom both coasts of America, the eastern and western, and thus shutting off the latter from the great Pacific Ocean, may at first appear unnatural. Yet it is supported by all facts bearing on the subject. There are no species known to be common to Western America and the Middle Pacific, excepting two or three cosmopolites. Moveover, the genera are to a great extent distinct, and where so, they often occur on both sides of the continent. The genera of Podophthalmia peculiar to America are mentioned on a preceding page, and also the particular coast on which they occur.

A review of some of the facts will exhibit in a strong light the zoological resemblances of the two sides of the continent.

Of *Cancer*, there are *four* species found on the west coast of South America, *three* on the west coast of North America, and *two* on the east coast of North America.

Of *Hepatus*, there is *one* species common to the West Indies and Brazil, a *second*, found at Rio Janeiro; a *third* at Valparaiso, Chili; a *fourth* on the Carolina coast.

Libinia, in the same manner, has its species on the Atlantic and Pacific coast of the United States, and the coasts of Western and Eastern South America. *Mithrax* is as widely distributed.

Epialtus occurs in the West Indies, California, Brazil, Gallapagos, and Valparaiso. *Potamia* has two West Indian and one Chilian species.

Eurypodius of Southern South America has its representative at Puget's Sound, in the genus *Oregonia.*

Again, the *Libinia dubia* of the West Indies, is hardly distinguishable, according to Prof. L. R. Gibbes, from the *L. affinis*, Rand., of the California coast. *L. spinosa* of Brazil, is also found in Chili. *Leptopodia sagittaria* occurs in the West Indies, and also, according to Bell, at Valparaiso; *Acanthonyx Petiverii* (?), in the West Indies, Brazil, and Gallapagos; *Epialtus marginatus*, on the coast of Brazil and at the Gallapagos (Bell); *Epialtus bituberculatus*, in Chili, and at Key West; *Uca una*, Guayaquil and West Indies; *Albunæa scutellata*, West Indies and San Lorenzo, Peru; *Hippa emerita* and *talpoides*, both on East and West America, North and South.

It is obvious, therefore, that the east and west sides of America are very closely related, and differ widely in a zoological sense, from either of the other kingdoms.

We observe further, that nearly all the genera peculiar to America are *cold-water* genera. They are mostly Maioids; the large group of the Cancroids, which belong mainly to warm waters, does not include a single genus exclusively American, and of the family Leucosidæ, of the Leucosoids, there are only three known species.

We also perceive why the western coast of America has no zoological affinity with the Pacific Islands. The temperature of their waters is widely different; and, moreover, the oceanic currents of the tropics run *from* the American coast to tbe westward, and are a barrier to migration eastward.

The relations of the American or Occidental to the Africo-European kingdom are of much interest. The two kingdoms are widely different in most respects.

In the first place, the genera *Lupa*, *Gelasimus*, *Ocypoda*, *Libinia*, *Epialtus*, *Hepatus*, well represented on the American coasts, are not known on the European, besides others (Table 1, on a preceding page) of less prominence.

Again, there are several genera common in Europe, not known in America, as *Inachus*, *Maia*, *Achæus*, *Portunus*, *Ebalia*, *Latreillia*, *Athanas*, in addition to those included in Table 2.

Still, the American and Africo-European kingdoms have a common character separating them from the Oriental. For example: the great genus *Cancer* occurs in both of these kingdoms,

and is not known in Oriental seas, except in New Zealand and Tasmania. So also the important genus *Homarus*; besides *Hyas*, *Herbstia*, *Leptopodia*, *Atelecyclus*, *Munida*, and *Grimothea*. The genus *Homarus* has one species on the coast of the United States, one on the coast of Europe, and one at Table Bay, South Africa, thus ranging over the whole Atlantic.

We may now treat separately of the several Kingdoms, and their subdivision into *Provinces*, pointing out the naturalness of their limits, and the characteristics of these Provinces. Each temperature region along a coast makes a distinct Province, which facts, where ascertained, show to be well characterized. In some cases, a further subdivision may be desirable, and when so, the subordinate divisions may be called *Districts*. In each Kingdom, the Provinces of each zone together may constitute a *Sub-kingdom*, as the *Torrid* Subkingdom, *Temperate* Subkingdom, &c.

Art. XVI.—*On the Grooving and Polishing of hard Rocks and Minerals by dry Sand*; by William P. Blake.

The phenomena about to be described were observed in the Pass of San Bernardino, (California) in 1838.* This Pass is one of the principal breaks through the southern prolongation of the Sierra Nevada, and connects the Pacific slope with the broad and low interior plain of the Colorado Desert. It is bounded on each side by high mountains; the peak of San Bernardino rising on the north to the height of about 8500 feet, and San Gorgonio, on the south to about 7000. The elevation of the summit level is 2808 feet above the Pacific, and the width of the gap at that point is about two miles: from this the ground slopes each way very gradually, the grade or descent on the east, for about 28 miles, being on an average, 69 feet per mile.

On this eastern declivity of the Pass—the side turned toward the Desert—the granite and associate rocks which form the sharp peak of San Gorgonio extend down to the valley of the Pass in a succession of sharp ridges, which being devoid of soil and of vegetation, stand out in bold and rugged outlines against the clear unclouded sky of that desert region.

It was on these projecting spurs of San Gorgonio that the phenomena of grooving were seen. The whole surface of the granite, over broad spaces, was cut into long and perfectly parallel grooves and little furrows, and every portion of it was beautifully smoothed, and though very uneven, had a fine polish. For a moment it was impossible to realize the cause of all this abra-

* A brief notice of these phenomena is given in the writer's Preliminary Geological Report, accompanying the Report of Lieut. R. S. Williamson, of a Reconnoissance in California, House Doc., 129, p. 27. Washington, 1855.

sion performed in a manner so peculiar; the action of glaciers and drift was thought of in succession; but the appearance of the surface was so entirely different from that of rocks which have been acted on by these agents, that I could not regard them as the cause. While contemplating these curious effects, the solution of the problem was presented. The wind was blowing very hard, and carried with it numerous little grains of sand. When I stooped down and glanced over the surface of the rocks, I saw that they were enveloped in an atmosphere of moving sand, which was passing over and accumulating in deep banks and drifts on the lee side of the point. Grains of sand were thus pouring over the rocks in countless myriads, under the influence of the powerful current of air which seems to sweep constantly through this Pass from the ocean to the interior.

Wherever I turned my eyes—on the horizontal tables of rock, or on the vertical faces turned to the wind—the effects of the sand were visible: there was not a point untouched, the grains had engraved their track on every stone. Even quartz was cut away and polished; garnets and tourmaline were also cut, and left with polished surfaces. Masses of limestone looked as if they had been partly dissolved, and resembled specimens of rock-salt that have been allowed to deliquesce in moist air. These minerals were unequally abraded, and in the order of their hardness; the wear upon the feldspar of the granite being the most rapid, and the garnets being affected least. Whenever a garnet or a lump of quartz was imbedded in compact feldspar and favorably presented to the action of the sand, the feldspar was cut away *around* the hard mineral, which was thus left standing in relief above the general surface. A portion however, of the feldspar, on the lee side of the garnets, being protected from the action of the sand by the superior hardness of the gem, also stood out in relief, forming an elevated string, osar-like, under their lee.

When the surface acted on, was vertical and charged with garnets, a very peculiar result was produced; the garnets were left standing in relief, mounted on the end of a long pedicle of feldspar, which had been protected from action while the surrounding parts were cut away. These little needles of feldspar tipped with garnets, stood out from the body of the rock in horizontal lines—pointing like jeweled fingers in the direction of the prevailing wind.

They form in reality a perfect index of the winds' direction, recording it with as much accuracy as the oak trees do, in the region about San Francisco, where they are all bent from the perpendicular in one direction, or in some places lie trailed along the ground. All these little fingers of stone pointed westward, in the direction of the valley of the Pass, to which the wind conforms. We experienced this wind before reaching the point of rocks and

the sand drifts: it blew with great force and seemed to be a great air current, as uniform in its direction and action, as the great currents of the sea. It flows into the interior with singular persistence and velocity, sweeping down over the slope of the Pass, not in fitful gusts and eddying whirls, but with a constant uniformity of motion unlike any of the winds of our Atlantic seaboard, or of the plains.

The Pass would in fact appear to be a great draught-channel, or chimney, to the interior, through which the air rushes inland from the cool sea, to supply the vacuum caused by the ascent of a column of heated air from the parched surface of the great Desert. This Pass is the only break of any magnitude in the mountain chain for a long distance, and as an air-channel, holds the same relation to the Colorado Desert as is sustained by the Golden Gate, at San Francisco, to the broad interior valleys of the Sacramento and San Joaquin.

The effects of driving sand are not confined to the Pass; they may be seen on all parts of the Desert where there are any hard rocks or minerals to be acted upon. On the upper plain, north of the Sand Hills, where steady and high winds prevail, and the surface is paved with pebbles of various colors, the latter are all polished to such a degree that they glisten in the sun's rays, and seem to be formed by art. The polish is not like that produced by the lapidary, but looks more like laquered ware, or as if the pebbles had been oiled and varnished.

On the lower parts of the Desert, or wherever there is a specimen of silicified wood, the sand has registered its action. It seems to have been ceaselessly at work and when no obstacle was encountered on which wear and abrasion could be effected, the grains have acted on each other, and by constantly coming in contact have worn away all their little asperities and become almost perfect spheres. This form is evident when the sand is examined by a microscope.

We may regard these results as most interesting examples of the denuding power of loose materials transported by currents in a fluid. If we can have a distinct abrasion and linear grooving of the hardest rocks and minerals, by the mere action of little grains of sand, falling in constant succession and bounding along on their surface, what may we not expect from the action of pebbles and boulders of great size and weight, transported by a constant current in the more dense fluid-water? We may conclude that long rectilinear furrows of indefinite depth may be made by loose materials, and that it is not essential to their formation that the rocks and gravel, acting as chisels or gravers, should be pressed down by violence, or imbedded in ice, or moved forward *en masse* under pressure by the action of glaciers or stranded icebergs. Wherever, therefore, we find on the surfaces of moun-

tains, not covered by glaciers, grooved and polished surfaces with the furrows extending in long parallel lines seeming to indicate the action of a former glacier, we should remember the effects which may be produced during a long period of time by light and loose materials transported in a current of air; and which consequently may be produced with greater distinctness, and in a different style, by rocks moved forward in a current of water. The effects produced by glaciers, by drift, or moving sand, are doubtless different and peculiar—so different and characteristic, that the cause may be at once assigned by the experienced observer, who can distinguish between them without difficulty. It is, however, possible that after a sand-worn surface, such as has been described, has been for ages covered with moist earth, a decomposition of the surface would take place sufficient to remove the polish from the furrows and leave us in doubt as to their origin.

If it were possible, it would be exeedingly interesting to ascertain the length of time it has required for the little grains of sand to carve the surface of the granite ridge to its present form. How inappreciably small must be the effect produced by a single grain! And yet by their combined and long continued action mighty effects are produced. That the action of the grains singly, is not visible, is proved to us by the polished surface, for no one grain cuts deeply enough to leave a scratch. Ages have doubtless elapsed since this action of the sand began, and we cannot tell how deep the abrasion has extended; cubic yards of granite may have been cut into dust and driven before the wind over the expanse of the Desert.

ART. XVII.—*The Vegetable Individual in its relation to Species*;* by Dr. ALEXANDER BRAUN.—Translated from the German by CHARLES FRANCIS STONE.

PART II.†

As I attempted to show in Part I, whatever seems arbitrary and indefinite in the existing views of what constitutes the Vegetable Individual has its ground in the nature of plants themselves, which, in their realization are resolved into a plurality which they are not capable of reducing to as complete an unity as animals are. As we ascend in the natural kingdoms, individ-

* Das Individuum der Pflanze in seinem Verhältniss zur Species,—Generationsfolge, Genrationswechsel und Generationstheilung der Pflanze, by Dr. A. Braun, Professor of Botany in the University of Berlin, &c. &c.

† For Part I, see Number for May, 1855.

uals increase in importance, until they reach their most perfect independence in Man. Hence, if we would appreciate them justly in the lower departments, in which their character is less definite, we must try to comprehend the less perfect structures by starting from the more perfect ones: to appreciate vegetable individuals we must start from a comparison of animal individuals. From this point of view we perceive at once that the cell cannot be regarded as the proper individual in plants, otherwise it would have to be considered in the same manner in animals. Cell-formation is a property common to plants and animals: but in animals it appears far more obviously as a subordinate element in the organization of the whole body, than it does in plants; since the animal cell, in most cases, is not so independent, nor so determinate, nor so permanently isolated as the vegetable cell. For this reason, too, it is rarer to find the animal cell considered as the proper animal individual, although *Schwann* has shown that animal cells are analogous to vegetable cells and may be as justly considered individual organisms as they. Yet as mere *curiosa* we might adduce the somewhat similar assertion of Gaillon, that "men and animals are properly masses of Infusoria;" and Oken's doctrine of generation, "a synthesis of Infusoria," might, perhaps, be interpreted in the same sense. The "stories" of the axes, the internodes with their leaves, might claim to be compared with the animal individual with more justice than the cell, especially if leaf-formation really took place as the defenders of such doctrines have represented: that is, if every successive leaf were produced as a *new* structure out of the old one (out of its base which becomes the internode), and if the whole stem were thus merely a concatenation of leaves shooting out of and growing above each other. But this is not so; the rudiment of the stem as an uninterrupted growth ("continuance") is formed *before* the leaves, while the latter, emerging as developments of the upper surface of the stem, are evidently members dependent upon and belonging to the axis, and forming with it one whole. Hence the structure of the internodes may be more aptly compared with the lateral structure of the animal body, and that of the leaves with its terminal structure. Thus we arrive at the shoot; and we must investigate the question, whether it should be considered as what corresponds best with the animal individual, or whether we must ascend still farther, up to the whole plant-stock.

The Shoot as the Vegetable Individual.

The first and most common view is that which considers the individual in plants, as in animals, to be merely each single specimen, i. e., each representative of the species which appears to be one whole from the connexion of its parts. To some extent

this view is correct, for in a forest of trees of the same genus and species, in a meadow, or in a cornfield, each single tree, each stock of grass or of grain, appears as a single member of its species, as each single beast does in a flock of animals forming a community. But the question arises whether these individual beings, regarded as such in this superficial way, can each be considered individuals in the same sense. When the flocks or societies of animals are numerous, as in an apiary, each hive or swarm will appear as an individual member of its species, and the more so in proportion to the closeness of the connexion between the members of such a community. Many flocks of animals whose members are organically connected during life, have until lately been considered to be individual animals; and even when the separation of the individuals is more complete, such conceptions are to a certain extent justified as long as the community is really a natural growth—when in fact it consists of members of one single stock—and we are not surprised to find that the oldest history of the human race describes the family itself, and the tribe which springs from it, as one person, named after its patriarch. As regards the plant-stock, even a superficial examination shows us peculiarities which will hardly allow us to consider it as an individual in the precise meaning of the term, and which calls upon us carefully to consider whether it is to be regarded as such an individual, or merely as an individual in the broader sense,—as one united family. Even our feelings aroused by the sight of the most ramified plant-stocks,—especially by a tree with its numerous branches, with the thousands of blossoms and fruits which it bears, and the numberless buds through which it will deck itself again in the following year with leaves and flowers—excite the presentiment that this is not one single being, one single life, comparable with the animal or the human individual, but rather a world of united individuals which have sprung from each other in a succession of generations, and although they do not separate, going through their particular cycles of existence,—here dying off, there reproduced, and thus building themselves up in uninterrupted succession into a family-tree, perennially laden with an increasing posterity. That such a view, so consistent with our healthy natural feelings, is corroborated by scientific investigation, I hope to show in the following observations.

Comparing plants with animal individuals, it is at once evident that the tree loses annually flowers and fruit,—the highest and noblest structures which vegetable life produces,—to generate them again in the following period of vegetation. Even the whole dress of the tree, even its foliage when compared with the trunk and branches, is only a superficial growth periodically dying off, and reproduced by the succeeding generation: in

the paradoxical words of Schleiden:* "No tree has leaves." The leaves, in fact, never grow out of the woody portions of the tree, but only on its herbaceous extremities, which grow upon the woody stem as upon a ground formed by the process of vegation. This common ground, namely the woody stem, which is almost lifeless in comparison with the herbaceous parts engaged in active growth, is annually covered with a vigorous sheath under the protecting bark, and this sheath is the ground of the nourishment of all the vegetating herbaceous extremities. This sheath is the so-called *cambium*, a layer of active, living tissue which, contemporaneously with the lignification of the herbaceous extremities of the branches, becomes a new woody layer, united to the old trunk in the form of an annual ring—to be covered in its turn in the following period of vegetation with a new layer, which, again, will be the immediate supporter of the new generations. The history of the grand development of nature on the surface of our globe presents an analogy which may perhaps serve to set this relation in a clearer light. The successive geological formations superposed during the course of countless ages, present, buried in their depths, the traces of as many formations of the organic world, each of which carpeted the then superior stratum of the earth with a new life, until it found its own grave in the succeeding formation, when a new uprising of organic life took its place. In the same way the stem of a tree is a multistratified ground, in whose layers the history of earlier growths are legibly preserved. The number of the woody layers indicates the number of the generations which have perished, i. e., the age of the whole tree; a distinct annual ring is the monument of a vigorous season, an indistinct one of a bad season, a sickly one (which is often found among healthy ones) indicates the unhealthiness of the foliage of that particular year. The practised woodman can decipher many facts of the past in the layers of the trunk, e. g., a good season for foliage or for seed, damage by frost or by insects, etc.

Essentially the same relations as those seen in the tree, or the shrub, are to be found in the subterranean perennial growth of plantæ redivivæ (herbaceous perennials) whose subterranean stem (rhizoma), like the stem above the surface, emits annually a new generation of herbaceous growths; whose stalks however, unlike those of the tree, do not lignify and form a part of the common supporter, but die off wholly, or mostly, at the close of the season of vegetation.

The relations indicated above compel us to recognise a succession of generations in trees, shrubs, and perennial herbs; and

* *Beitr.*, p. 152, where the following view of the arboraceous stem, as a common ground bearing many individuals is developed; but this whole view, after all, needs to be corrected by a precise limitation of its meaning by what follows it.

thus our first idea of them as individuals is necessarily modified. Another remark may be made here which confirms our idea thus modified. Natural *death* closes the life of the individual.* The development of the life of individuals in organic nature has a goal, an acme; after it has attained this goal its course draws to an end. This is not the case in the tree and the perennial herb. True the tree is destroyed by time; but this seems to result more from external, and in part mechanical causes, than from any internal decrepitude. The more numerous the generations which the tree builds up, one above the other, the greater is the distance of the growing extremities from the source of their nourishment; the thicker the supporting trunk the thinner is the layer of cambium which connects the new shoots with the extremities of the root by which the nourishment is absorbed. This increased difficulty of communication between the upper and lower extremities is probably the cause of the decrease of vigorous growth after the plant has arrived at a certain age. But in most cases external casualties are superinduced, which accelerate the termination of the tree's life. It is injured by wind and weather, the decay of the injured part spreads through the whole organism, various fungi fix themselves upon the tree, and are especially fatal when they attack the roots. Oftentimes the tree breaks down under the weight of the productions of its own vital powers, the luxuriance of its fruit. These statements are corroborated by the cases of trees of unusual age, now so well known through De Candolle's investigations. One of the examples adduced by him shows in particular that those trees whose branches have been prevented from breaking down by props or supports attain to a great age. I refer to the celebrated Linden in Neustadt on the Kocher, which, as early as 1229, was the cause of the town's being called "Neustadt an der grossen Linde" (Neustadt of the great Linden), whose wide-spreading branches were supported already in 1408 by sixty-seven stone pillars, and this number was afterwards increased up to more than one hundred.† The hoary tree still flourishes, having survived its many scientific admirers, among whom was my predecessor, to whom Botany is so greatly indebted, who visited and described it a few years ago, (in 1849).‡ Natural

* Cf. *Schleiden: Beitr.*, p. 151. "The idea of individual life necessarily implies as its distinguishing characteristic individual death, preconditioned in the organization itself." Although this remark is not universally true in many respects, yet I have adopted it for the light it is calculated to throw on the nature of the tree. For the very reason that natural death is the result of a determinate conclusion of the development, those shoots (vegetable individuals) which have no such conclusion frequently undergo no death at all except that of some of their parts: but this is a concomitant of animal life itself, (casting the skin, moulting, and the organic changes in the body). Cf. on this point Roeper: Linnæa, 1826, p. 439, and the following remarks on *Paris*, *Lysimachia nummularia*, *Adoxa*, etc., and the preceding ones on *Caulerpa*.

† *De Candolle: Physiol. Veg.* II, p. 988.

‡ *Link:* Erinnerungen an die grosse Linde bei Neustadt am Kocher (Flora, 1850, No. 8).

supports are more efficacious in preserving trees than even artificial ones; since they not only prop the branches, but conduct nourishment to them by a shorter road, as is actually found to be the case in *Rhizophora Mangle*, in various species of figs, [Banyan, &c.], and other tropical trees, whose branches high in air send down strong roots into the earth. A similar example nearer home, though indeed on a much smaller scale, is found in the *Juniperus Sabina.* Its branches, which spring from a low stem, curve down to the earth, strike numerous roots, and raise themselves again, so that the comparatively feeble stem may carry a creeping crown of considerable extent, like a thick wood continually spreading, and which may continue to flourish in its parts, even when the communication between the original supporter and nourisher of the whole colony and the succeeding new growths, which are constantly receding from it, has finally ceased. A remarkable specimen of this tree stands in the Royal Botanical Garden at Schöneberg, which, if not as old as the Garden itself, which was laid out in 1679 under the great Elector, Frederic William, certainly dates as far back as Gleditsch's time, and his directorship commenced in 1744. The main stem is not more than 33 inches in circumference at eight inches above the ground, close under the place where the first branches originate; the centre-piece of the crown which belongs immediately to the stem, is only nine feet high, and has been dying off during several years, while the maximum diameter, from S. W. to N. E., of the hundred-rooted crown, which has spread out over the ground by the declination of the branches, measures 35 feet; the entire circumference of the crown, which amounts to about 100 feet, would be still more considerable if it had been permitted to spread on every side, and if the branches on the N. E. side had not been removed at an early day.

What has just been said of trees admits of no doubt as regards perennial herbs (plantæ redivivæ) with subterranean creeping stems or stolons. Such plant-stocks as those of the well-known *Paris*, *Anemone nemorosa*, *Convallaria majalis*, *Asperula odorata*, are undoubtedly exposed to none but a casual death.* All plants which renew the cycle of vegetative life repeatedly and without any determinate limits to their existence, and which I would hence call *anabiotic*, cannot therefore be considered *simple* individuals.†

* The same relations of great unlimited age are found in polyps which form stocks. Cf. Ehrenberg: *Abh. d. Acad.* for 1832, p. 382, 420, where among others, stocks of Mæandriæ and Faviæ are referred to, larger than a cord of wood—which may readily be supposed to have been seen by Pharaoh.

† I pass over the further question, intimately connected with this subject, whether the composite plant-stock itself, with all its subordinate generations, with all its possible divisions,—viz., the individual in the most comprehensive sense (in which Gallesio conceived it) has not a determinate term of life, though not easy to be ascertained, on account of the narrow space of time accessible to our direct experience.

At first sight the case seems to be different in the *haplobiotic** plants, which terminate their existence at the end of the simple process of development, with the formation of flowers and fruit; and this they do whether they exist one year, as *Adonis æstivalis* and *autumnalis*, *Nigella*, *Papaver Rhœas*, *Erigeron Canadensis*,† or for two years, as *Oenothera* and *Verbascum*, or for many years,‡ as *Agave* [Century-plant], the East Indian *Corypha*, and the Mexican *Fourcroya*,§ which suddenly puts forth its flowers only after 400 years of extremely slow growth, and ends its life with the formation of its first and long-deferred fruit. The development of these plants, when compared with that of the first mentioned anabiotic plants, seems at first to comprise only *one* generation, and to depend upon the development of *one* individual. But here, too, a closer examination shows conditions incompatible with the nature of the simple plant (the individual). One constituent element in the idea of an individual is, that the parts of the organism are *essentially* connected; yet the stock of annuals themselves presents a multitude of parts which bear no essential relation to the whole plant. This is true of a large part of the ramifications, of branches which may exist in one case and not in others, and

* De Candolle calls *anabiotic* growths *polycarpic* and *haplobiotic growths monocarpic*, terms which are useless from their ambiguity. With an equally inappropriate choice of terms he divides the first (Phys. Veg., II, p. 73) into *caulocarpic* and *rhizocarpic*, according as the stem which produces the fruit is permanent, or dies off down to the root; but the latter in fact never takes place in perennial growths; for in such cases the life of the plant-stock is preserved not by the mere root alone, but by a subterranean portion of the stem. It is one of the most remarkable confusions which a want of true biological ideas has engendered, that De Candolle should have regarded the simplest and most natural circumstance in the plant's life,—its death after having attained the goal of its development,—as an unnatural and to some extent casual occurrence, as a kind of sickness comparable to the succumbing of the mother in childbed, which he accounts for by the rapaciousness of the flowers and seeds. Röper, however, in a note to his translation of the above work, justly remarks that there are annuals with double flowers which die off to the ground although they produce no seeds. We may convince ourselves beyond a doubt that the flowers, on the contrary, are much less rapacious than the vegetative parts of the plant, that they even shut themselves off from the afflux of too copious nourishment; for many plants develop vegetative branches close under the terminal flower, as e. g., *Stellaria media*, *Datura*, *Mirabilis*, etc. In such cases the flower-stalk, which cuts itself off from almost all farther afflux of nourishment, remains slender, while the portions of the stem directly beneath, and the branches which spring from it, gorged with succulent matter, enlarge more and more, and attain a most disproportionate size.

† These plants, like other annuals which germinate in the autumn, are usually reckoned among biennials; but this is a mistake, for like our winter corn they are *plantæ annuæ hiemales*. So, too, many vernal plants, as *Teesdalia*, *Erophila*, *Cardamine hirsuta*, *Spergula Morisonii*, and many weeds of the winter corn, e. g., several species of tares, *Bromus secalinus* et aff.

‡ *Corypha umbraculifera*. Cf. *Rheede*, *Hort. Mal.*. iii, pl. 1–12. This is also the case in the palm-genera *Metroxylon* and *Eugeissona*, according to Martius (*Hist. Palm*, I, p. 108).

§ On *Fourcroya longæva*, cf. Zuccarini in the Nov. act. nat. cur., xvi, 2, p. 666 and pl. 48.

which are proved to be unessential by the plant's losing no essential function when deprived of them. For even when the plant does not produce them, it can fully consummate the object of its individual life; it can produce flowers and fruit. A glance at the examples just now adduced, *Nigella*, *Papaver Rhœas*, *Adonis*, etc., will make these statements obvious. The branches of these plants, each of which, like the stem, is crowned with flowers and fruit, are evidently only *unessential repetitions of the simple plant*, absolutely identical with the main stem, and hence to be ranked as equal to it in importance, i. e., equally to be viewed as particular individuals, and with as much reason as in Zöology we concede individuality to the branches of the coral-stock (polypidom), which are now universally acknowledged to be individuals, and which offer an analogy of decisive importance for ascertaining the nature of the branch in vegetables. In view of this analogy Ehrenberg regarded plants as aggregations of individuals.*

We can now turn back, and apply what has been shown to be the case in the annual herb to the shrub and the tree, each of whose annual generations now appears more distinctly than before to be, in their peculiar connexion, not one individual, but a world of individuals developing in the same period of vegetation and upon the same stem. To this intent many of the early botanists have expressed themselves, as I stated in the Introduction. Thus, B. Batsch, e. g., says of branches, that they shoot forth from the stem "as if they were so many plants rooted in it;"† and Goethe:‡ "lateral branches may be regarded as particular plantlets which are rooted upon the maternal stem, just as this stem is upon the earth." Among moderns, Unger, at the close of his investigations into dicotyledonous stems, says . . "Buds and the branches they develop are individual plants, which live by preying upon the maternal stem."§ Similar expressions are used by Schlei-

* *Abh. d. Akad.*, 1835, p. 247. . . . . "Hence a polyp-stock is a mass of animals. We have no satisfactory comprehensive expression for our idea of a plant. What an individual is remains still unknown; most of them are evidently aggregates of individuals which may be compared with coral-stocks." . . . The origin of coral-stocks is minutely described by Ehrenberg in the Abhandl. for 1832, where he makes the following remarks: "The coral structure is neither a mere structure composed of many animals arbitrarily conjoined, as Ellis supposed; nor one single animal with many heads, or with simple furcations, as Cavolini maintained; nor a vegetable stem with animal flowers, as Linnæus expressed it; it is a body of families, a *living* tree of consanguinity, the single animals belonging to it, and continually developing upon the primary ancestor, are entirely isolated within themselves and capable of complete independence although unable to achieve it."

† *Bot. für Frauenzimmer*, p. 15–16.

‡ *Versuch d. Metam. d. Pfl. zu erklären*, p. 59. The words "just as" in the passage quoted imply too much, and remind us of Du Petit-Thouars' unfounded doctrine of the formation of the woody layers of the stem by the 'roots' of the buds which penetrate it.

§ *Ueber d. Bau u. Wachsthum des Dicotyledonenstammes*, p. 177. Here, too, "preying" is too strong a term.

den;* they are most definite in Röper's works.† Linnæus expressed the same thought in the words "*gemmæ totidem herbæ.*" And I am thus led to make a particular remark, which is intended at the same time to modify in some degree what I said before in relation to the annually renewed generations of trees. It is indeed true that branches of trees and perennial herbs, especially in temperate climates, first appear as buds; and in a more extended sense we call in general every young branch a bud, even if its parts are not, as they usually are, compactly arranged and folded together; still, all buds are not the rudiments of branches. *Lateral* buds are the only ones from which branches originate, and therefore they alone are to be regarded as new lines of development,—as individuals. Terminal buds, on the contrary, are nothing but still undeveloped parts of the (relative) principal axis: they are mere continuations and augmentations of the individual already existing, and are not to be regarded as commencements of a new one.‡ Hence, only those trees which produce no terminal buds, as the Linden, Willow and Elm develop new individuals and nothing else at each renewal of vegetation; while, on the contrary, those which do produce terminal buds also, as for example the Oak and Poplar, bear a mixed annual generation, which consists partly of new individuals, partly of old ones reawakening and continuing their development with renewed vigor.

I have already remarked how unessential the presence of branches is in many plants. A comparison of stocks grown on a rich soil with those of a poor one shows what license is given to plants in regard to producing branches, and how different the appearance of specimens of the same species thus becomes. Plants grown on a poor soil are often called dwarfs; but unjustly, for they present the most normal development of all essential parts, dispensing with everything that is unessential, and are much less

* *Grundz.*, ii, p. 4. "New identical individuals develop upon the maternal stem by continuing the growth," etc. Here the expression "continuing the growth," is improper, for the shoot does not "continue" the growth at all, but is a new growth from a new rudiment.

† "Omnis gemma solitaria aut ejusdem continuatio immediata et perpendicularis (caulis, ramus, ramulus, flos) individuum vegetabile vocatur." This is the most definite description I know of; for in this passage not only the branches so-called, but also every arbitrary shoot, even when it is merely a flower, is acknowledged to be a particular individual. Besides what I have stated in the text in regard to the appearance of terminal buds, I have only to remark, against the word "gemma," that in its growth every shoot does not enjoy a perceptible state of gemmation, i. e., a state of rest in which its parts are folded together. The term bud is applicable to but one state of a shoot or of its parts, and therefore cannot be a suitable expression for what is to be regarded as the vegetable individual.

‡ *Kutzing*, (*Phil. Bot.*, ii, p. 146,) aptly expresses these relations by calling the terminal bud the continuation of the "series of formations," lateral buds beginnings of a new "series of generations." In contradiction with these terms, however, he calls the bud an "organ" as long as it is connected with the natural individual—a term inapplicable to the bud as it is to the developed branch, of which it is the adolescent state.

inclined to malformations than the lusty giants of the rich soils. Not unfrequently we find diminutive speciments of *Erythræa pulchella s. ramosissima* which are branchless and perfectly simple, as they terminate with a flower after four or five pairs of leaves. More vigorous specimens produce two branches out of the axils of the highest pair of leaves, which after a single pair of leaves terminate in the same manner with a flower; and branches of the second order may be also emitted from the axils of the two leaves preceding this flower; and so on. In the first order of ramification the number of flowers amounts to three, in the second to seven, and so on; in the seventh, which is not unfrequently attained, it amounts to 127! Here, if we would consider the stock or specimen as the individual, and the flower as the superior termination of the vegetable organism, comparable, say, to the head of the animal, this variation in the number of the flowers would be as astounding as if we were to learn that an animal might have 3, 7, 15, 31, 73, or 127 heads, according to circumstances. The same thing occurs in *Radiola linoldes*. *Erigeron Canadensis*, which often grows to the height of a man and bears as many branches as a tree, presents dwarfed specimens scarcely two inches high and of a perfectly simple form.* After developing two early deciduous cotyledons it presents about 13 leaves on the stem, which are followed by a terminal capitulum of 21 involucral bracts and about 34 flowers. One middle-sized specimen about three feet high presented nearly 100 branches of the first order, out of which branches of the succeeding orders proceeded, together bearing about 2000 heads, and hence (reckoning the head at 34 flowers) 68000 flowers.†

I may here remark, that such unessential branches may be separated and reared independent of their parent stem; on which fact depends propagation by artificial divisions, which is so variously employed in horticulture. The most remarkable case of this artificial division is recorded by Miller: in the year 1766–67, he obtained 500 stocks of winter Rye, by dividing one stock and repeating the operation three times; these 500 stocks emitted 21,109 spikes, bearing together 576,840 grains. Nature herself, as well as art, in various ways may effect such an independent separation of developed branches or of undeveloped buds, and this too either above or beneath the ground. Propagation of the Strawberry by its runners; of the Potatoe and the *Helianthus tuberosus* by their tubers; of bulbous plants by their bulbs; of the

* Not counting the florets, which also are properly so many branches.

† Similar cases occur in most annuals. The forms of *Bromus mollis* and *racemosus* with simple spikelets instead of rich panicles, are well-known; less known and less remarkable are the depauperate specimens of *Umbelliferæ* with one single unifloral umbel, some of which of *Scandix Pecten* are in my possession. I have also specimens of *Solanum nigrum*, one and a half inches high, with a solitary terminal flower.

Garlic by the bulblets formed in the process of flowering, and falling off like seeds; of the varieties of the beautiful *Achimenes* by the amentaceous or the strobiliaceous deciduous shootlets, are well-known examples of this process; and thousands of others might be adduced.*

The gardener can not only separate individuals, but unite them upon one stem. This is true not only of individuals of the same species, but even of those of different species; sometimes even of different genera of the same family. The Lilac is not unfrequently grafted upon the Privet (*Ligustrum*), the Pear upon the Mountain Ash (*Sorbus Aucuparia*), the Peach upon the Almond. By the insertion of a bud (inoculation), or of a developed sproulet (grafting), we are thus enabled to pluck different kinds of roses from the same bush, to gather different kinds of fruit from the same tree. It would evidently be a contradiction in this case to consider the whole tree, or the whole bush, as the individual; for we should then give the name to a compound of several species, or even of several genera.

In attempting to comprehend the vegetable individual in its simplest form, we have thus far spoken of unessential branches only, and have endeavored to show that they cannot be regarded as mere parts of the individual. But there is another kind of branches, those which are essentially requisite for the attainment of the end of vegetation,—for the formation of flowers and fruit. These occur in all plants which possess no terminal buds, and which must hence necessarily have some branches in order to attain the end of their existence. This is the case with the Evening-Primrose, Larkspur, *Orchideæ*, etc., whose lateral flowers are just such essential branches. If we demand that the individual should be a complete representative of the characters of the species, as is implied in the usual view, then we must add to the principal axis such branches as these,—without which the process of vegetation is not concluded, and on which, in fact, the most essential and characteristic parts of the plant make their appear-

* I will only adduce a few more of these examples, which might be multiplied indefinitely. Besides the Garlic (*Allium sativum*) in many other species of *Allium*, e. g., *A. oleraceum*, *carinatum*, *vineale*; *Lilium bulbiferum*, *tigrinum*, *humile*, and other species, *Gagea fistulosa*, *Ficaria ranunculoides*, *Dentaria bulbifera*, *Saxifraga bulbifera* and *cernua*, *Cicuta bulbifera*, *Polygonum viviparum*, *Begonia bulbifera*, *diversifolia* and other species, *Remusatia vivipara*, *Cystopteris bulbifera*—buds fall off above the ground (as bulblets). In *Stratiotes aloides* rosette-like developed axillary shoots separate close to the base. The separation of lateral shoots in *Lemna* is well-known; and it occurs in a similar manner in *Pistia*, by the separation of thin-stalked lateral rosettes, and in *Hydrocharis* in the separation of peculiar winter buds. When the inferior leaf-formation is gorged with sap, bulblet-like buds form from the axils of the root-leaves (frondes fundi) in *Saxifraga granulata* and many exotic species of *Oxalis*, in the same way as the bulb-brood of monocotyledonous bulbous plants. Inferior leaf buds which are placed on the ends of their stolons become free by the death of the runners in *Epilobium palustre*, *Lycopus Virginicus*, etc., and swell out and form little lumps. Cf. on this subject Wydler (Flora, 1853, p. 17—24).

ance,—and call these parts of the same individual. In this sense Schleiden's view of the simple plant might perhaps be justified, although, as he starts from different premises, he does not consider mere floral branches as particular individuals. He says: "If nothing but organs of reproduction, or flowers, spring from the bud we still call the plant a simple one."*

Here, however, we arrive at a contradiction, which shows us that we cannot carry out the idea of the vegetable individual with the requisite definiteness in this way, since we thus regard essentially similar branches, now as individuals in themselves, now as mere parts of individuals. As I have already remarked, Schleiden allows individual importance to branches which are identical† with the main axis; those on the contrary which produce flowers alone, and in this respect differ from the main axis, he regards as mere parts of the simple individual. This distinction when analyzed is perfectly nugatory; since it only lays down two extremes, between which there are an infinite number of gradations. Strictly speaking, there are no branches which are perfectly identical with the main stem, as is evident from the fact that no branch begins with cotyledons, as the main axis does.‡ Besides, the foliaceous leaves on the branch are almost always fewer than those on the main axis, and generally fewer in proportion as the point is higher where the branch originates. The arrangement of the leaves on the branches, also, often differs from the arrangement on the main axis, as e. g., in most of our broad-leaved trees,—in the Elm, Hazle, Chestnut, Linden, etc., in which the phyllotaxis on the main axis, and often at a later period in the so-called "water-shoots" (Wasserschossen), is spiral or decussate, while on the branches, it is, on the contrary, distichous. In *Alnus viridis* the phyllotaxis is tristichous on the main axis, and distichous on the branches. On the main axis of Cypresses and *Thuja* there are 3–4-leaved whorls; on the branches the pairs of leaves are nearly decussate; this is also the case in *Lysimachia vulgaris*. In the same way in *Equisetum* the number of the rameal verticillate leaves is always inferior to that of the cauline ones. While thus on the one hand the vegetative branches are nowhere entirely similar to the stem from which they spring, on the other hand it appears that those branchlets which seem to bear flowers only are usually more numerous than they seem to be; since in most cases one, two, or even more small leaves (bractlets), are present beneath the flower, which may easily escape notice on account of their diminutive size, although their existence may

* *Grundz.*, ii, p. 4.

† *Grundz.*, ii, p. 4.

‡ The basillary cotyledons of the branches, indeed, have been compared to cotyledons. This comparison is partly justified in view of the commencement of phyllotaxis on the branch; which often resembles that on the main axis, whilein regard to form and consistency almost all resemblance disappears.

be often ascertained with certainty even in those cases in which they are not visible when the flower has reached its complete development.* If we are to deny individuality to those buds (branches) only which are composed of a flower alone, as a strict interpretation of Schleiden's language demands, we should have to draw a most unnatural and often impracticable line of demarcation between branches which, physiologically speaking, are perfectly homologous (floral branchlets which really have no bracts), and those which bear imperceptible or even suppressed (abortive) bracts. If on the other hand we would reckon the latter also among the branches which are not individuals, then it may be contended that there is such a series of gradations in regard to number and vigor in the leaves which precede the rameal flower, that it is impossible to draw a dividing line even in this manner.

The above-mentioned distinction between unessential and essential branches seems to afford a better stopping-place, no matter

* In fact, all the constant lateral flowers of *Primulaceæ*, *Cruciferæ*, *Capparideæ*, *Resedaceæ*, *Balsamineæ*, *Orchideæ*, never have any bractlets. Among monocotyledonous plants in many cases there is only one bractlet; among the dicotyledonous there are generally two. *Gesneriaceæ* have generally three; *Empetrum* and *Santalum* have four, *Eriostemon* five, *Polemoniecæ*, *Cuscuteæ* and other plants with paniced inflorescence an indeterminate number. We possess the following means of showing the existence of suppressed bractlets: 1. The position of the parts of the flower relatively to the axis of origination from which the lateral flowers spring. 2. Analogy. 3. The study of malformations. 4. Observations of the flower's development. The first criterion can be applied only where we can determine the succession of the parts of the flower. The position of the parts of a lateral flower depends in fact upon determinate laws of rameal origination; when they do not harmonize with these laws we must conclude that preceding leaves have been suppressed. In this way, e. g., we can explain the very common position in the 2-5th arrangement of the calyx with the second sepal posterior, by supposing two bractlets according to the fixed law, while it cannot be explained without these bractlets. Analogy aids us most by confirming our conclusions, as e. g., in the families *Scrophularineæ*, *Labiatæ*, etc., in which many genera present distinct bractlets, while others appear to be without them. In monstrous flowers (in cases of *antholysis* and *chlorysis*), sometimes without any other malformation, bractlets otherwise imperceptible appear in an abnormal growth. Not unfrequently in *Digitalis purpurea*, which in its normal state presents no bractlets, but in which we inferred their original existence from æstivation and the position of the calyx relatively to the axis, I have found bractlets developed in the most heteregeneous degrees, especially on the lowest flowers of the raceme of cultivated specimens. C. Schimper and myself have both observed the same fact in *Tropæolum majus*, which, like most species of this genus, presents no trace of bractlets in the normal state. We have seen them in the form of very small, white, subulate leaves about in the middle of the flower-stalk, while the flower remained unchanged in all other respects. Their existence, however, was already indicated by the position of the quincuncial calyx relatively to the axis, as well as confirmed by analogy, for *Tropæolum ciliatum* R. et. P. (Pöpp. et Endl. Nov. Gen. t. 38) in its normal development has two round and prettily ciliated bractlets on the the flower-stalk. I have mentioned the history of development last, not to disparage study, but because the morphology must be rightly understood beforehand by means of comparisons of developed structures, and because in its present stage the development is incapable of giving us reliable information in regard to all the leaves which are present in the germ, though they may not develop. To know what parts then exist we should have to be able to distinguish the leaf as a cell or a group of cells before it rises to view above the surface of the stem.

whether the branch bears nothing but a flower or not. We might say, all essential branches must be regarded as individuals since they repeat the process of specific development laterally, and can become independent plants, as layers, whether natural or artificial. Those branches, on the contrary, which appear as necessary members in the line of development which is advancing towards flower and fruit, and which therefore complete the series of formations belonging to the species, and without which the plant is either unable to eke out its vegetable life or to accomplish propagation, must be regarded as members of one and the same history of development. Let us take a case where the main stem bears only proper leaves, branches of the first order only bracts, and those of the second order only flowers and fruit, as is really the case in *Plantago*, *Melilotus*, *Veronica officinalis* and *Chamædrys*; here it is evident that these three divisions cannot be isolated; that all three must necessarily be present in order that the specific life may attain a complete representation in one individual.*

Notwithstanding the importance of this discrimination between essential and unessential branches, it cannot, when analyzed, establish a distinction which will enable us to decide upon their importance as individuals; for even those branches which appear unessential, in relation to the formation of flowers and fruit, may yet be essential to the plant in other relations: as when they appear as characteristic elements of the vegetable structure, or when they play any important part in the economy of the plant, as I have shown *in extenso* elsewhere.† Nay, more; one and the same branch, as to whose nature there seems to be no doubt, may appear either as essential or as unessential, according to circumstances. When those branches which conduct the structure to a higher stage of its development appear in great numbers on a principal axis, as e. g., in indefinite racemose or spicate inflorescence, the lateral branchlets appearing as flowers are then indeed, generally speaking, necessary to the plant's full completion of the series of formations, and in this sense essential; but their number is immaterial as regards this completion; and this the plant itself shows in producing either a larger or a smaller number of them; sometimes the number is reduced to one.‡ Therefore, properly speaking, only *one* lateral flower is essential: and we may arbitrarily consider any one of the number to be this essential one. Hence each of them may be regarded indifferently as essential or unessential. This is not the case in those racemes and spikes which possess a terminal flower, as is the case in many

* [But why assume (as here and *supra*) that the species must attain a complete representation in a single individual in vegetables?—since this is by no means the case in the higher (unisexual) animals, where there is no doubt as to what corporeally constitutes the individual,—that is, in the very cases whence we derive our idea of individuality, and the standard of comparison which our author is endeavoring to apply to the case of plants. A. G.]

† v. Verjüngung, p. 41, et. seq.

‡ E. g. not unfrequently in the raceme of *Lathyrus odoratus*.

Campanulacæ, e. g. in *Campanula rapunculoides*. Here all the lateral flowers are unessential: yet if the terminal flower is cut off, the lateral branchlets which bear the flowers at once become essential. Such a change is not always artificial, for it often happens naturally, as there are plants in which the terminal flower may be either present or absent. *Agrimonia Eupatoria*, and *Campanula rapunculoides*, are examples of this variability.*

We can cut this Gordian knot only by deciding to consider every branch as an individual, however appearances may be against it, provided that we have other grounds sufficient to regard branches as individuals. The genesis of branches justifies us in so doing; for each branch is not a direct continuation laterally, is not a development belonging to the stem (like the leaf), but is a new formation; like the main axis itself, it has its own centre of formation, with its peculiar development. Branch and stem, main axis and lateral axis, differ therefore only in their origin and relative position; but they are essentially of the same nature; they are united in the idea of the *shoot*. The stem is the primary and principal shoot of the whole plant, the branch is a lateral shoot in reference to the main shoot; but it can itself become a relatively main shoot, and the stem of a succeeding generation of shoots in its turn. As far, then, as we are justified in speaking of vegetable individuality at all, we must hold fast to the individuality of the shoot: *the shoot is the morphological vegetable individual*; is that form or that part of its specific realization which is analogous to the animal individual, if any part is.

In zöology we give the name of individual to every whole which is controlled and bound together from one vital centre. Since such an internal domination of the organism as that which characterises animal life is wanting in plants, whose existence is a process of growth directed externally alone, we can only demand, as the criterion of vegetable individuality, that the individual shall be formed in direct continued development from one centre, and thus in accordance with its origin, *shall, in all its parts, belong to one centre.* Now this is the character of the shoot. Its centre of formation has been known since C. F. Wolff's celebrated "Theoria Generationis" (1759) under the name of "*punctum vegetationis*;" it is about what is called in

* *Agrimonia Eupatoria* bears usually one spike without any terminal flower; in weak specimens, a terminal flower not unfrequently makes its appearance which opens before the upper lateral flowers. This has been observed by Wydler (Bot. Zeit. 1844, p. 642). In *Campanula rapunculoides* the case is just the contrary: its looser spikes are usually terminated with a flower, while denser ones end in a coma of bracteal leaves, without any terminal flower. *Dictamnus* resembles *Agrimonia*; while *Triglochin* (especially *Tr. maritimum*) on the other hand imitates *Campanula*. Even in plants in which the essentiality of the lateral position of the flower is expressed by their zygomorphic development, terminal flowers make their appearance in some cases; they then resemble *Peloriæ*. This is the case in *Linaria*, *Orobanche*, and a *Digitalis purpurea monstrosa* (described by Vrolik, Flora, 1844, No. 1), which propagates by seeds, and is now widely disseminated in our gardens.

common life the "heart," of the plant, or, at the first appearance of the lateral shoot, the "eye." The whole future of the plant slumbers unseen within it; leaf after leaf arises out of it, step by step, at a measured pace, prescribed by law, until (in case the shoot is destined to conduct the development thus far) the series concludes with the last formation, that of the carpels, which close over the dying point of vegetation and form the fruit. In this progress the centre, always keeping the lead, is ever advancing, rising more and more, and leaving behind it an axis arrayed with the organs already formed. Hence we may designate the vegetable individual as *the sum of the parts belonging to one axis.* Just as the body of the animal has only one trunk and one head, the shoot has but one axis and one apex. As the trunk of the animal has a second extremity opposite to the terminating head, and gradually dwindling down till it forms the tail, so the perfect shoot has a second extremity opposite to that which terminates with the most perfect structure (the fruit), and dwindling down to an indeterminate end, the root, by means of a *punctum vegetationis* turned downward.*

But it will be objected: is not the vegetable shoot indefinitely divisible, can we not cut it up into an arbitrary number of pieces, each of which is capable of reproducing the whole plant in its turn? Were this the case the phenomenon would not be without its parallel among the lower animals. But this is not the case. The supposed divisibility of the vegetable shoot, at least in perfect plants (the Phanerogamia) to which I am now alluding, is a delusion, which rests simply upon the fact that the formation of new shoots has been confounded with a reproduction of the shoot as such. As the injured shoot has the faculty of producing new shoots, so the parts of the divided shoot have also this faculty in many cases; but this is no re-completion of the shoot itself; the fragment of the old shoot can continue to develop in one single case only: when, in fact, it bears the apex of the axis with the point of vegetation. Let us examine this case more closely. If a shoot is divided transversely, under certain circumstances the upper part, on which the *punctum vegetationis* ("the heart") is

* Aristotle, on the contrary, considered that the root, being the imbibing organ, was the part of the plant which corresponds to the upper part, to the head and mouth of the animal; and he regarded the stem as the inferior part. He found the cause of this topsy-turvy position of plants in the necessity under which they labor of drawing their nourishment from the earth, as they are incapable of moving from place to place. In this respect he compares plants to muscles (ἐστραμμένα), which also have their heads turned downwards. Cf. *Wimmer: Phyt. Aris. Frag.* 56–65. This comparison of the root with the animal's head is however, morphologically speaking, inverted; for as the highest stratum of the spinal chord (the sensorial portion) attains its maximum state of development in the head of animals, it can only be compared to that extremity of the plant's axis in which the highest and noblest part of the plant is exhibited. Besides, the peculiar and striking characteristic of the animal's head, its involved structure terminating the organism, is by no means to be found in the root end of the plant; but it is seen in the opposite end which terminates with flower and fruit.

still remaining, may continue the development; but the lower part is nothing but a stump, aud continues to be a stump which can never complete itself by a terminal shoot, and which never fails to die if it is not nourished by lateral sprouts formed before, or sometimes after, the division took place, and thus kept alive by its posterity. This cannot be called divisibility, in the usual meaning of the term; the whole phenomenon, on the contrary, strongly reminds us of the capacity animals possess of losing the less essential caudal extremity without any cessation of life. In favor of this view the fact may be adduced that a similar phenomenon occurs in the normal process of development of plants and animals. As there are animals which may spontaneously lose the posterior extremity of their body during the course of their development, as e. g., *Cercaria*, *Comatula*, frogs, etc., so there are also numerous plants in which the posterior extremity gradually dies off, and is cast aside, during the course of growth, while the anterior end of the shoot, which bears the *punctum vegetationis* continues to unfold; as is seen in the growth of many mosses, especially of Peat-mosses, in the creeping and climbing rootstocks of Ferns and *Aroideæ*, in the long creeping stems of *Lysimachia nummularia*, the little subterranean creeping rootstocks of *Paris*, in most plants which possess a *radix præmorsa*, as e. g., *Succisa pratensis*, the perennial species of *Plantago*, in *Tormentilla*, etc., with which the perennial bulbs of monocotyledonous plants agree in all essential respects; and finally, this is especially remarkable in *Utricularia*, and in *Selaginella increscentifolia*, whose apices only form close buds, and last through the winter, while all the remaining parts of the shoots perish. If the shoot is indivisible transversely, it is still less so longitudinally. There is not a single case to prove that a shoot longitudinally divided can as such continue to develop; nor do we know of a single case where such a longitudinal division takes place spontaneously. What has been usually described as a bifurcation of the stalk depends in the Phanerogamia in every case upon a true ramification which takes its rise laterally close under the apex, as I have already described it in the case of *Erythræa pulchella*. As a *normal* formation no immediate division of the stalk occurs among Phanerogamia; for the phenomenon known as "fasciation," which might be adduced here, is always a monstrosity.* The stalk, or axis of the shoot, is hence indivisible in

* Fasciation depends upon a real division of the punctum vegetationis into two parts of equal importance; in the simplest case it produces a simple division into two parts. Here neither of the two parts can be regarded as a branch of the other. If repeated bifurcations follow each other in the same plane, and in unbroken connexion the well known "ribbon and fan" like forms arise which however usually end at last in single apices. Very rarely more than two parts lying in different planes are produced by the division of the *punctum vegetationis*, a case which I have noticed in the capitula of Compositæ. The rarest phenomenon which bears upon our subject is the annular fasciation, in which an annular border arises from the simple point of vegetation, of which I shall speak more at large in the following Part, when

the higher plants, in the same sense that the body of the higher animals is indivisible.* The only phenomenon which might be described as a division of the stalk is leaf-formation. This, however, is not a division into new stalks, but a formation of subordinate parts belonging essentially to the stalk, as it were an eradiation of the stalk itself, which may be aptly compared to the formation of the extremities in the animal body. We may therefore justly describe the shoot, or the vegetable individual, as an indivisible axis,—as an axis with its appertinent radii which are inseparable from, and regularly arranged by, its own development. With the first appearance of the branch a new axis is formed, and a new system of subordinate radii appears. However completely the branch may contrive to interweave itself with the trunk during the course of its development, it always owes its origin to an accessory point of vegetation which develops into a particular axis. The vegetable individual thus presents in its nature a certain analogy to the mineral individual,—the crystal,—as well as to the animal individual; for the crystal is determined by the relation of its parts to one and the same system of axes. As soon as this system of axes holds another position there results another individual, which may be distinguished even when two or more individual crystals intersect, so as to form twin crystals, or stellate crystals.

In the preceding considerations on the indivisibility of the axis I described the leaves as its radiations,—as members of the stalk, and belonging essentially to it,—and I attempted to distinguish the leaves from the branches, by considering the latter as new axes. But how are leaves and branches distinguished in their genesis? Are not the branches as much radiations or lateral members of the stalk as the leaves? It would lead me too far from my subject to make a fundamental critical investigation into this question, and to examine the existing views of the mode of formation of leaves and branches, especially as investigations into this subject have not been complete enough to enable us to obtain reliable results. I can therefore only allow myself a few hints in this place. The leaf originates in the earliest period of the formation of the stalk; and its rudiment is contemporaneous with the first stages of the formation of tissue in the *punctum*

I compare the relations of growth in the Cryptogamia. A division of the individual corresponding to fasciation in phanerogams and to dicthotomy, its homologue, in many cryptogams also occurs in the animal kingdom, as appears especially in many genera of corals, e. g. *Caryophyllaea* whose stocks are formed in this manner exclusively, and in *Astraea* and *Faria* in which it appears in conjunction with shoot-formation (gemmation), as was shown by Ehrenberg, (Beiträge, etc., Abh. der Acad., 1832. p. 242). Ehrenberg explains the form of *Dadaline* as a result of incomplete termination of the individuals in gemmation; in appearance it resembles the coxcomb-like forms of fasciation as they occur in a remarkable way in some monstrous *Cacti* of the genera *Mammillaria* and *Echinocactus*, as well as in *Celosia cristata*, well-known as an ornamental plant.

* [Some criticisms upon this may be given at the close of the whole memoir. A. G.]

vegetationis. A leaf can never be formed at a later period from the developed axis. It is a necessary consequence of the manner in which the leaf originates, that an absolute dividing line cannot be drawn between leaf and axis; for the subsequent position of the leaves upon the organism affords no standard of appreciation, especially as most of them do not mark the basis of the leaf, which loses itself in the axis. Earlier, before the extension of the axis begins, the rudiments of the leaves are always closely pressed together, so that they appear as a peripherical development of the axis itself, occupying the whole upper surface, and dividing it into clearly defined planes, which may be recognised even in the developed state, in those plants whose foliaceous *pulvini* are distinctly marked, as e. g. in many Ferns, most acerose plants, in Cacti, and particularly in *Nymphæa* and *Victoria* where the *pulvini* may be distinguished even in the interior of the axis. The primitive vascular system of the axis enters directly into the leaves, and ramifies there; while the woody layers of the stem, which are found later, have no connexion with the leaves. With branches the case is totally different. In their origin and development they always succeed the leaves; and even at a much later period, when the leaves have been long cast off, shoots may originate in places where, at an earlier period, no trace of a rameal rudiment, or of an eye, was to be found. If we now consider the axillary shoots,—i. e. those branches whose position is predetermined by the situation of the leaves,—at an early period we shall find their rudiments, even though they develop very late or not at all, in the form of a circular and slightly prominent gibbosity, which may be compared with the apex of the axis; or rather, it is an accessory *punctum vegetationis* forming near the apex. The circumstance of the epidermis of the axillary shoot's being a continuation of that of the stem, is explained by the early date at which it originates; for this takes place at a time when the surface of the axis has not yet lost its flexibility. The eye is shown to be an independent centre of vegetation by its subsequent internal and external conformation; for it not only develops leaves upon its surface, and this too with an independent commencement of its phyllotaxis, but even in its interior the first system of vascular fibres seems to be formed independently of that of the main axis; as originally it lies upon it, and afterwards becomes intimately blended with it by later layers of tissue. Notwithstanding the intimacy with which later formations of woody tissue unite branch and stem, still, according to Unger's investigations, no immediate influence is exerted by the branch upon the conformation of the stem, since the stem owes none of its essential parts to the branches.* This independence of the branches is shown still more decisively in adventitious shoots, whose posi-

* *Unger: Ueber den Bau des Dicotyledonen-Stammes* (1840), p. 65, *et* 66.

tion is not predetermined by the leaves. Originating at a later period, they take their rise not from the surface but from the cambium layer,—the internal tissue which preserves the faculty of producing new growths. Hence if they would come to the light of day they must break through the bark. Their origin has been particularly described by Trécul.* W. Hofmeister, however, as I have already remarked, succeeded in tracing it in *Equisetum* back to the first cell, a cell in the interior of the stem. As is the case with axillary buds, such adventitious buds sometimes remain undeveloped for a long time (ten years and more) without losing their vital activity; a fact to which attention has lately been called by C. Schimper,† in a report on exostoses. When this is the case they not unfrequently develop into spherical or conical wood-kernels, which continue to exist without any connexion with the ligneous body of the maternal stem; this is especially the case in Beeches and Poplars.

The individual nature of the shoot is confirmed not only by the mode, but by the place, of its origin. While the organs of the individual organism,—the leaves of the plant,—occupy a position determined with geometrical accuracy, shoots on the contrary can arise out of almost any part of the plant, wherever indeed any cambium exists; and they may be even enticed by art out of places where they do not usually appear. There are shoots from the *stem*, the *root* and the *leaves*. In herbaceous stems they appear in situations determined by the leaves (in the axils of the leaves), while they may be found anywhere on old woody stems‡ as adventitious buds, or on any part of the lignified roots of most dicotyledonous woody growths, and even on some monocotyledonous ones, as in *umbraculiferæ*.§ Shoots appear less frequently on the roots of herbaceous plants.‖ Shoot-formation from leaves has often been discussed and described in regard to many plants, especially *Bryophyllum*, *Cardamine pratensis*, *Drosera*, *Malaxis paludosa*, etc. A fine example of this is shown by a *Chelidonium majus var. laciniatum* reared by Bernhardi in the Botanical Garden at Erfurt, from whose leaves

* *Trécul: Recherches sur l'orig. des bourg. adv.* Ann. des sc. nat., viii, (1847) p. 268.

† In Sept., 1852, in the Versammlung der Naturforscher in Wiesbaden.

‡ Rarely scattered shoots appear on the herbaceous stem, and especially on the first internode under the cotyledons, as Röper (Enum. Euphorb. 1824) first showed in *Euphorbia*, and Bernhardi in the germ of *Linaria*. A specimen of *Begonia manicata, dipetala* cultivated in our [Berlin] Botanical Garden, which is probably the same species as the *B. phyllomaniaca* of Martius, presents the case of a plant which produces a multitude of shootlets in the whole leaf-region; they arise from the sappy stem which is not yet hardened, soon after the fall of the leaves.

§ According to Rheede, *Corypha umbraculifera* sends forth root-shoots when the stem dies off after the fruit has ripened.

‖ I have often observed them in *Linaria vulgaris*, *Helichrysum arenarium*, *Rumex Acetosella*, *Ajuga Genevensis*, *Jurinea Pollichii*, *Nasturtium sylvestre* et *Pyrenaicum*. According to Wydler, they often appear in *Viola sylvatica*.

floral bractlets arose, partly unifloral, partly multifloral, without any preceding leaves.* Shoots may be allured by the gardener out of most leaves which do not wither too soon.† Finally, the little budlets in whose bosom the germ of the new plant is formed and developed, and which we call seeds, are a kind of shoots, which in most cases owe their origin to leaves, (carpels) out of which they spring (on the margins, which unite to form the placenta) or more rarely, out of their whole inner surface.

(To be concluded in our next number.)

Art. XVIII.—*On Different Centers of Primitive Civilization;* by Thomas H. McLeod.

We have already called attention in a previous number of the Journal of Science‡ to the radical difference of the Indian, Grecian and Roman Systems of Numerical Notation. We would again refer to those several Systems, and in addition, to the Egyptian, Mexican and Chinese, in reference to different centers of primitive civilization.

It will be remembered that the Indian System commences with a 0 (zero), and is made up of principal and subordinate measures, and has its origin probably in land or lineal measuring; that the Grecian System is a system of principal and subordinate measures also, but has no 0 (zero), its first figure being a *unit*, making use of mere characters to express its large measures, and has its origin in the contemplation of individual objects; that the Roman System also has no 0 (zero), its first character being a *unit*, and is rather a system of fives than of tens, introducing a new character (V) for that number, one for ten (X), which may be regarded as a double V, one for fifty (L), one for one hundred (C), one for five hundred (D), and one for one thousand (M), expressing the intermediate numbers by repetitions and combinations. The Egyptian System commences with a *unit* which it represents by a single downward stroke, adding an additional stroke for each successive number to ten which it denotes by a triangular figure (△); this figure is repeated for each successive ten to one hundred. The scheme will be seen by the following representation:

	10	20	30	40
I, II, III, IIII, IIIII, IIIIII, IIIIIII, IIIIIIII,	△,	△△,	△△△,	△△△△, &c.

* I may add to the examples I have given of shoot-formation taking place out of the leaves, one which I observed in June, 1853, in *Levisticum officinale*. I found, in fact, in several species of this Umbellifera, one or more, frequently two, shoots in the points of division of the leaves, which after producing a few weak leaves bore a small umbel. (Later note.)

† *Kirschleger* (Flora 1844, No. 2) notices a fine example of this in *Gloxinia speciosa*.

‡ Journal of Science for January, 1855, Art. vii.

"The Aztecs expressed the digit *one* by a small circle, thus, O. Months were expressed by a peculiar symbolic figure for each, taken from natural or artificial objects. Duplications and combinations of the circle were made adjacent to the symbol so that the day of the month was exactly denoted. These were also the formulas for other transactions."* The Chinese System consists of thirteen distinct characters and is a system of juxtapositions. The *unit*, which is the first figure in the system placed adjacent to a certain character denotes one hundred, two units placed with this character denotes two hundred, &c.

From this statement it is manifest *first*, that, the Indian, Chinese, Egyptian, Mexican, and probably the first system of the Greeks are radically different and primitive; *secondly*, that, the Roman System and the second system of the Greeks, which is similar to the Roman, is a simplification of, and derived from, the Egyptian; and *thirdly*, that these several systems originated in the very earliest periods of the human race before the several stocks of these nations had made any advancement in common, in civilization, and after they had separated from each other, for they could not have advanced far in the cultivation of the arts and sciences without a method of numerical notation,—not even from the savage state; for the savages of North America were beginning to form a rude system, when they were first known to the Europeans, so that it may be justly inferred that the several nations with whom the several systems of notation above alluded to were formed, separated before they had advanced in civilization beyond the North American Indians. Consequently there must have been several points on the earth distant from each other, where the mental energies of man were developed and primitive civilization began. These points, as evinced by the above facts, were probably, the East Indies, Egypt, possibly Greece, Mexico and Central America, and China.

Art. XIX.—*Additional Note on Arachis hypogæa;* by George Bentham, Esq.†

In the year 1838 a short paper of mine was read before the Linnæan Society on the structure and affinities of the *Arachis*, in which I pointed out the curiously imperfect achlamydeous female flowers from which the fruits are produced, whilst the apparently perfect hermaphrodite flowers are, generally speaking, if not always, barren, and I showed a closely similar structure and fructification in *Stylosanthes*, next to which I proposed to place *Arachis* among *Hedysareæ*. This paper was published in the eighteenth volume of the "Linnæan Transactions," a work

* Extract from a private letter from Hon. H. R. Schoolcraft.

† From Hooker's Journal of Botany, No. 77, 177.

which is unfortunately far too expensive and bulky to have any circulation among foreign botanists. The conclusions I had come to became known to them only by abstracts contained in botanical journals or other compilations, unaccompanied by the observations from whence they had been deduced; and my proposal for associating *Arachis* with *Hedysareæ* has been more than once treated as absurd, without however any facts or arguments being brought forward in opposition. Recently again a writer in "Silliman's American Journal," Mr. Hugh M. Neisler, whose article is reproduced in the last number of "Taylor's Annals of Natural History," adduces some observations of his own in support of a denial of the existence of the two kinds of flowers in *Arachis*, although he also had not seen my paper, the details of which would probably have led him to perceive his mistake. At the time I wrote it I had only had dried specimens to examine, but these were numerous and good, belonging to several species of *Arachis*, and to about twenty species or marked varieties of *Stylosanthes*. I have since then repeatedly examined dried specimens of both these genera, as well as of *Chapmannia*, and have observed *Arachis hypogæa* in a living state, especially in the summer of 1853, when I had the opportunity, in the Botanic Garden at Leipzig, of rooting up and carefully examining several plants of that species, bearing a profusion of flowers of both kinds, in various stages of development. These flowers always appear several together, in short, close spikes, in the axillæ of the leaves. In the upper axillæ, the barren but apparently perfect flowers are the most numerous; but even these are generally accompanied by one or more of the minute fertile ones, and the latter, which are always without calyx or corolla, become more numerous in the lower axillæ. The withered perfect flowers remain long sticking about the spike, and may sometimes be found apparently adhering to (but not connected with) the point of the fertilized ovary of the female flower, and borne along with it as its stipes lengthens, as mentioned by Mr. Neisler; but I always find within the tube of these withered flowers their own dried up, barren ovary, with its unfertilized ovules, and if Mr. Neisler will compare these barren ovaries with those of the female flowers before the stipes has lengthened above a line or two, he will find the latter very different in shape, smaller in size, with a small sessile stigmate, wholly incompatible with the supposition of its ever having borne the long filiform style of the barren ones. The presence of imperfect flowers, deprived of corolla and even of calyx, but more prone to form their seed than the more showy and perfect ones on the same plant, is a phenomenon of not unfrequent occurrence among *Leguminosæ*, especially in several genera of *Phaseoleæ* and *Hedysarcæ*, and has also been observed in other Natural Orders, such as *Cistineæ*, *Violaceæ*, *Malpighiaceæ*, etc.

Art. XX.—*Observations on Binocular Vision*; by Professor William B. Rogers.

PART SECOND.

Conditions under which two or more right lines are optically united.

In what precedes I have pointed out the unusual combination of visual adjustments by which from two separate drawings we form a simple binocular resultant, and I have shewn that this is not confined to experiments like those with the sliding-stage apparatus, but occurs, although to a less extent in the use of the common stereoscopes. The observations which I am about to describe have been made chiefly with the former instrument because of the facility it affords for multiplying and varying the experiments and because after a little practice it enables us to obtain the resultant figure as clearly and perfectly as the best stereoscope of Wheatstone's or Brewster's construction.

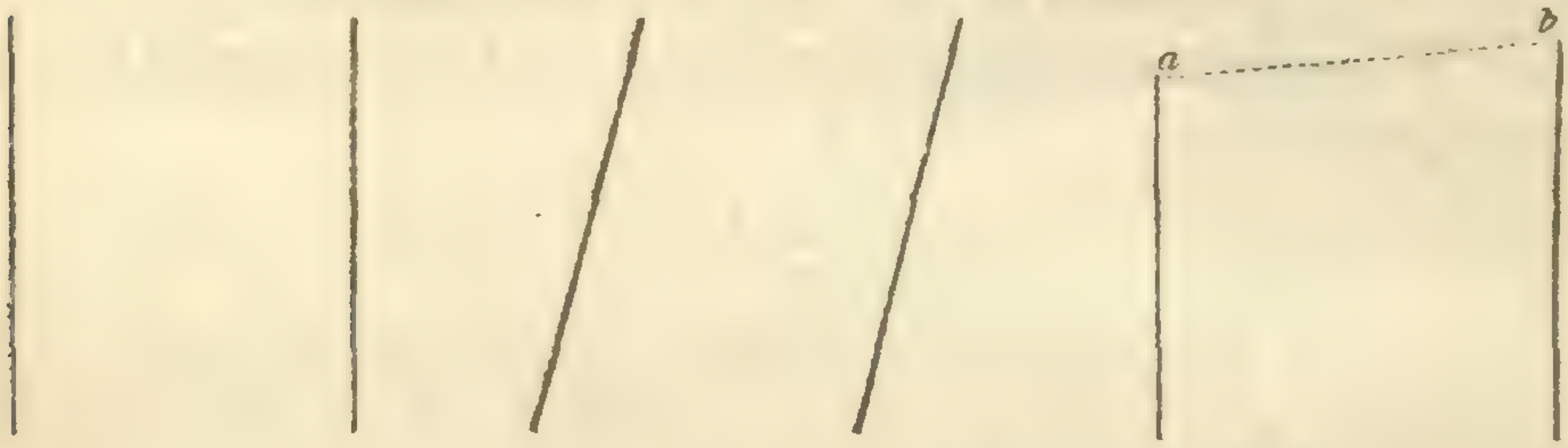
The purpose of these observations has been chiefly to acquire clearer ideas in regard to two important questions in binocular vision which have been ably investigated by Wheatstone and Brewster and on which they have published differing views. The first of these relates to the *process of forming the binocular resultant*, which, according to Brewster, always involves the successive combination of pairs of corresponding points in the two drawings, but according to Wheatstone, is simultaneous throughout at least a large part of the figure. The second question refers to the binocular *combination of unequal drawings* first described by Prof. Wheatstone, but the reality of which Sir D. Brewster has attempted to disprove by experiment.

In these enquiries I have begun with the most simple combinations and have thence proceeded to less obvious ones, endeavoring at each stage to repeat and vary the observations of the distinguished observers above named, and to reach some satisfactory conclusion where they differ from one another as to the fact or the interpretation. While these enquiries have in many respects satisfied me of the correctness of the views of these philosophers, they have led me to regard in a different and somewhat new aspect certain parts of the process of binocular vision involved in the questions above alluded to.

6. *Of the combination of parallel right lines either vertical or oblique.*

Two equal parallels (fig. 9 or fig. 10), when placed on the upper stage of the stereoscope may be united in a position farther from the eyes than their true place, or when adjusted on the

lower stage may be combined in a position nearer than their true place, and in either case the resultant is a single line parallel to the components and including them co-extensively throughout its whole length.



Unequal parallels (fig. 11) may also be united into a parallel resultant, but in this case the component lines are not co-extensive throughout the resultant. By marking their extremities with dots or short lines we can observe the limits of each, and we then perceive that as long as the eyes and the diagram preserve their relative position unchanged, the two lines are not coincident, even when they are very nearly of the same length. But by turning the head or the diagram so as to rotate the plane of the optic axes in relation to the figure, we may cause either the upper or the lower ends of the parallels to coincide according to the direction of the movement. When the upper ends are thus brought together the lower ones are seen to recede from one another; when the lower ends are made to coincide the upper ones separate to their greatest distance—the interval in both cases being marked by the dots on the resultant. The dotted line (fig. 11), shews the direction parallel to which the line joining the two eyes must be placed in order by a due convergence of the optic axes to unite the points *a* and *b*, and the angle made by this line with the horizontal measures the angular movement of the head or the diagram necessary to bring about the coincidence.

When the parallels are nearly coincident, the adjusting motion of the head is so slight as to be made almost unconsciously. This movement is, I think, one of the causes of the apparent union of unequal figures described by Prof. Wheatstone, and in that connection will be referred to in a subsequent page.

7. *Vertical rotation of the eyes associated with their converging movement.*

My experiments with unequal parallel lines have led me to observe effects accompanying the convergence of the axes to points either beyond or within the limits of distinct vision which indicate a rotation of the eyes in opposite vertical directions. As similar phenomena are noticed by a friend whom I have induced to repeat the experiments, I infer that they do not arise from any ab-

normal adjustment of my own eyes, and I therefore deem them worthy of mention in this place.

In attempting to unite unequal parallel lines by converging the axes beyond the plane of the diagram, I find it easier to bring about the terminal coincidence above described, when the lower of the dots is presented to the right eye than when opposite the other. The effect is very obvious when fig. 12 is adjusted on the

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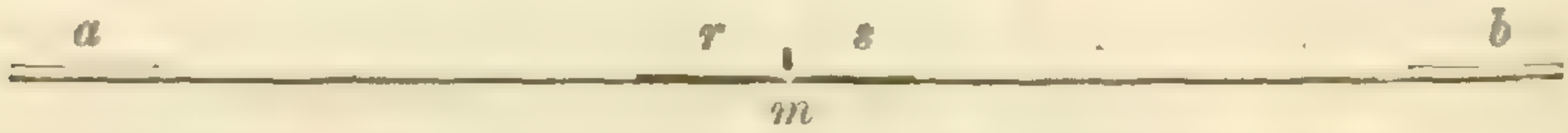

upper stage of the stereoscope, so that *a* and the right half of *m* are presented to the left eye, while *b* and the other half of *m* are opposite the right eye. Fixing the long line in a horizontal position in the plane of the optic axes, I cause *a* and *s* to approach, and in doing so I find that they either unite at once, or are readily brought together without shifting the head or paper. But when I make the same effort with *b* and *r*, I can only bring the latter below the former, and find it requisite to turn slightly the head or the diagram in order to unite them. It would therefore seem, that when converged beyond the limit of distinct vision, my eyes tend to revolve vertically in special directions, the right eye turning very slightly downwards and the left upwards.

When I direct my eyes to a point much nearer than the limit of distinct vision the evidence of vertical rotation in connexion with the converging movement is very remarkable. To observe this I adjust the lower and upper stages of the apparatus to the distance of 12 and 7 inches respectively and placing (fig. 13) on

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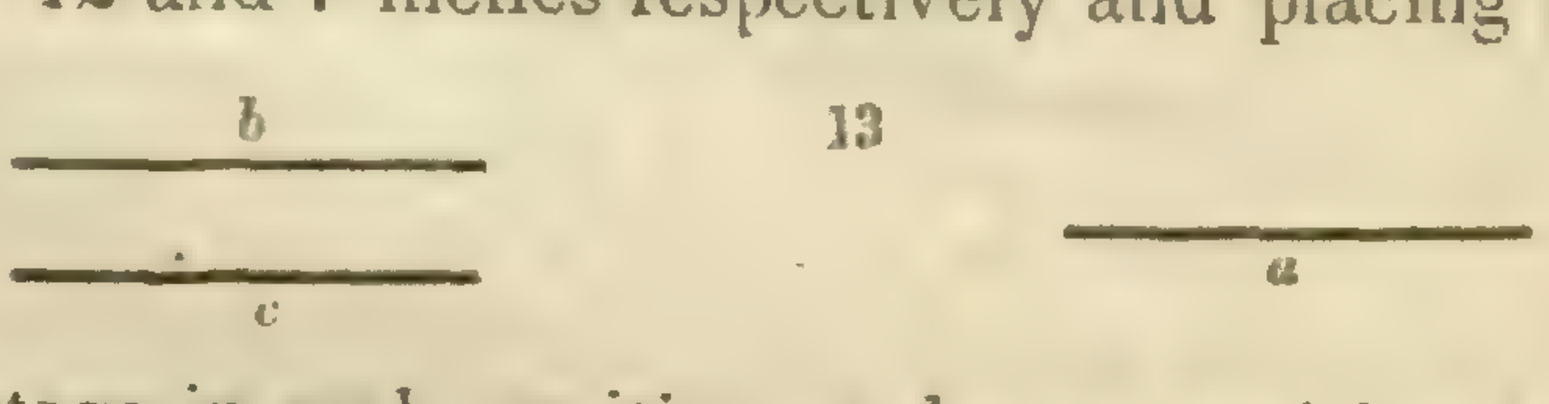

the lower stage in such position as by cross vision to present *b* and *c* to the right eye and *a* to the left, I converge the axes a little below the aperture. As the two parts of the figure approach one another, I see *a* and *b* coming nearer to the same horizontal level and when the due convergence of the axes is attained I observe them to coalesce completely. But retaining the figure and the head in the same position, I find it impossible to bring about a union of *a* and *c*. In order to do this I must either turn my head so as to depress the left eye below the level of the other, or I must turn the diagram in the opposite direction. When this latter motion is used to produce the coincidence of *a* and *c*, I find on subsequently inspecting the figure that I have been compelled to depress *a* considerably below the level of *c*.

A like effect appears when I view (fig. 14) as in the preceding experiment. In this case *a* and *b* instead of combining takes the

positions shown in (fig. 15), where the line presented to the left eye appears in a higher position than that presented to the right, although when directly looked at they are seen to be on the same level. By inclining the diagram so that *a* (fig. 14) shall be lower

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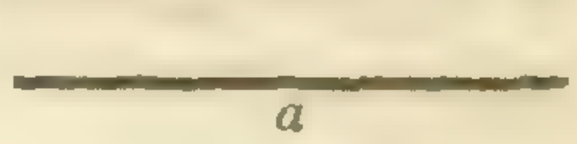

than *b*, the resultant parallels (fig. 15), approach one another, and when the proper inclination is attained, (in this particular case about eight degrees,) the two lines unite to form a single inclined resultant.

The following experiment, very easily repeated is one of many of the same kind which I have made to test the peeuliar adjustment of my eyes when strongly converged. Placing myself in a seat about 15 feet from a window, with my eyes on a level with one of the horizontal bars, I direct my view to the end of a pencil or other small object held before me at the distance of distinct vision. I then observe whether the two images of the horizontal bar form one line, and if they do not, I adjust the position of my head until I find them entirely free from vertical displacement. Without moving my head I now converge the optic axes to some point much nearer than the pencil, and I see the two images of the horizontal bar at different levels, the right image being lower than the other.

In all these cases of convergence to a point nearer than the limit of distinct vision the effect is obviously such as would arise from a rotation of the right eye in an upward and of the left eye in a downward direction. It thus appears that in converging my eyes towards a point beyond the limit of distinct vision, there occurs a very slight displacement of the two images in opposite vertical directions, indicating a motion of the left eye upwards and of the right eye downwards, and that in converging them to a point nearer than the same limit a much greater displacement occurs corresponding to a much more considerable rotation of the eyes in the reverse directions.

In referring the various effects above described to an opposite vertical rotation of my eyes, I would not have it understood that I regard such rotation as absolutely demonstrated in these cases. The same displacements might perhaps be explained by changes in the shape of the eyes due to muscular tension, but the rotary movement seems to furnish a much simpler and more probable explanation.

8. *Combination of pairs of vertical lines.*

If two pairs of equidistant and equal vertical lines (fig. 16) be placed on either stage of the stereoscope so that *a* and *b* shall be

presented to one eye and *c* and *d* to the other, it is easy by a proper convergence of the axes to unite *a* with *c* and *b* with *d*. The two resultant lines will of course be vertical and will be in a plane parallel to that of the diagram, and they will be situated behind or in front of it according as the union has been effected by converging the eyes beyond the diagram or before it.

When the intervals of the vertical lines forming the respective pairs are unequal (fig. 17), the union of *a* with *b* and of *c* with *d*

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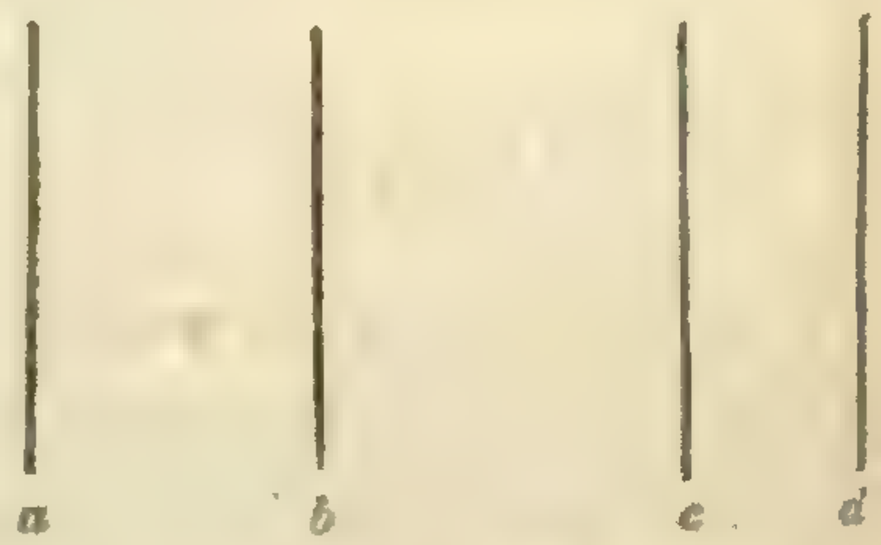

forms two resultant verticals which lie at different distances before or behind the plane of the drawing. Supposing the union in the present case to be effected behind that plane, the resultant of *a c* will be more remote than that of *b d*. If the combination is produced in front of that plane, the resultant of *a c* will be nearer to us than that of *b d*.

When the intervals between the verticals are nearly equal, the change of convergency requisite in passing from one resultant to the other is so quickly brought about that we can scarcely perceive that the two resultants are not seen each in perfect singleness at the same moment. If however the difference be great, as when *a b* is one inch and *c d* half an inch, although we can readily unite *a* with *c* and *b* with *d*, a considerable effort is necessary in passing from the one convergency to the other. Hence as soon as we form a distinct single resultant of *a c*, that of *b d* becomes double, and when again *b d* are united into one line the resultant of *a c* becomes double. The same effect occurs when the inequidistant vertical lines form parts of any of the twin drawings of a more complex kind which are usually provided for stereoscopes, as will be pointed out under a subsequent head.

9. *The two parallel resultants are formed at the same time.*

It is important to remark that when the pairs of parallels are equidistant, the two resultants are *simultaneously* formed, and do not require the eyes to be directed to them successively. This is proved by the fact that, however intently we fix the view on one of them, the other appears clearly visible as a single line, and when the attention is directed to a point between the two, the singleness of each is perfectly preserved.

Sir D. Brewster, in one of his valuable papers on binocular vision, maintains that in order to see an object distinctly the optic

axes must be successively conveyed on every point of it. In the case of objects having relief, and in that of the perspective figures of the stereoscope, I am convinced that this process is, to a certain extent, necessary. But the observations above cited show that the principle of successive vision does not apply in the case of parallel resultant lines in a plane at right angles to the direction of view, or in that of actual lines or points situated in that plane; it being understood in both cases that the objects are near enough to the optic axes to be distinctly visible. In a more recent paper, (Phil. Mag., May, 1847,) the same eminent observer appears to admit the latter view; for, in his beautiful observations on the binocular combination of the series of plane figures upon carpets and paper-hangings, he says: "these figures being always at equal distances from each other and almost perfectly equal and similar, the coalescence of any pair of them, by directing the optic axes to a point between the paper-hangings and the eye, is *accompanied by the coalescence of every other pair.*"

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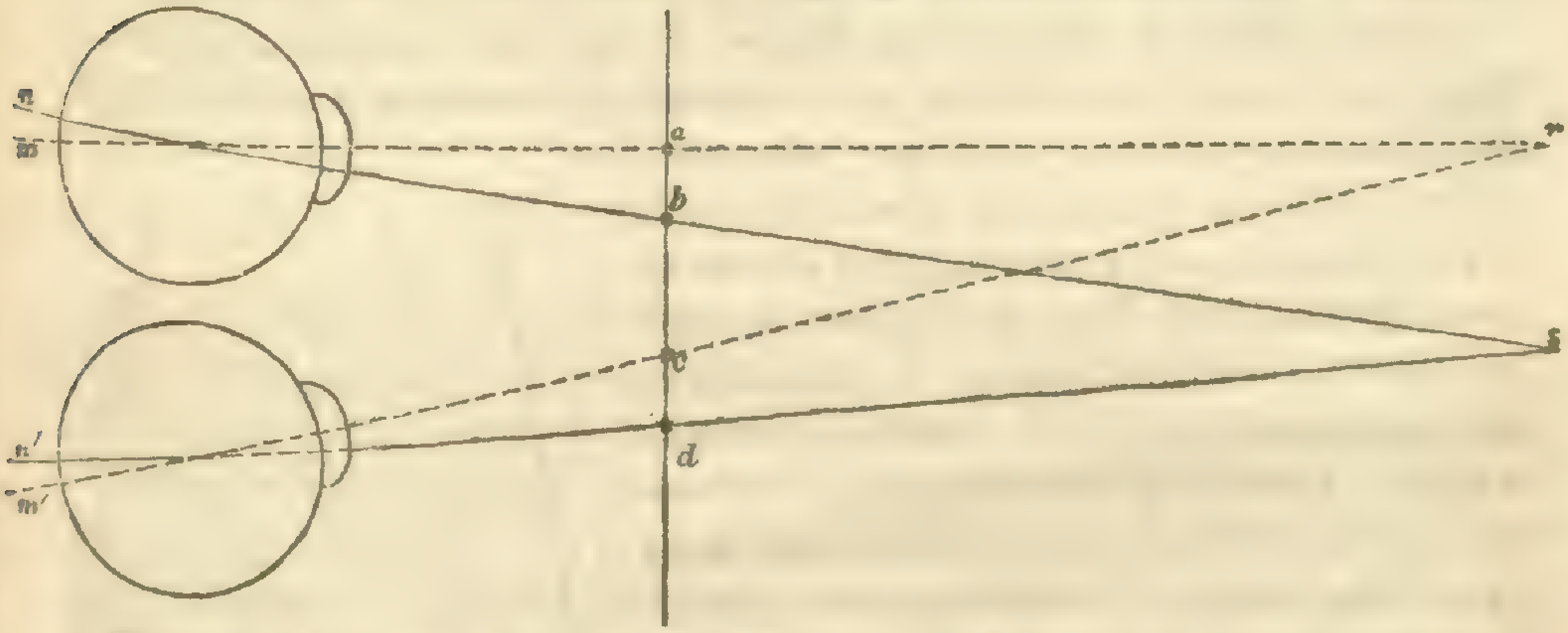

The conditions of this simultaneous binocular union are illustrated in (fig. 18), where *a* . . *b* and *c* . . . *d* represent the two pairs of equi-distant points, or parallels. Here *a* and *c* are united at *r*, the point to which the optic axes are supposed to be conveyed, and at the same time *b* and *d* are combined at *s*. As *a b* and *c d* are equal, *n* and *n′* are practically at equal distances from the retinal centers *m* and *m′*, on the same side, the normals to the surface at these points, marking the directions in which *b* and *d* are perceived, form at the resultant *s* an angle equal to that at *r*, and hence *r* and *s*, the two resultants, are practically at equal distances from *a d*, or from the line joining the centres of the two eyes.

10. *Conditions of vision of a physical line in a vertical position.*

When a straight wire, or other physical line, is held in a vertical position before the eyes, the visual conditions under which it is

seen as a single line are very simple. The optic axes being converged to one point of it, the images of the point formed in the two eyes are referred to the point itself, and as the same convergence serves a like purpose for the pairs of images corresponding to other parts of the line, the union of the two images of the line is *simultaneous* throughout so much of the length as is distinctly visible. When the line is of considerable length, a sensible change of convergence is necessary in tracing it upwards or downwards, and in these directions it assumes the character of a perspective line, the visual conditions of which will be referred to hereafter. Thus the *optical signs of a vertical line are uniform convergence of the optic axes and a simultaneous and perfect union of the two images of the line.* It is obvious that the same characters belong to all short lines situated in a plane perpendicular to the plane of the optic axes. But lines oblique to this plane, or in other words, perspective lines, are seen under very different visual conditions, the consideration of which belongs to the following section.

(B) 11. *Of the binocular union of mutually inclined lines.*

If we place on the upper stage of the stereoscope a card on which are drawn two lines converging upwards at a small angle (fig. 19), so that *a c* may be presented to the left eye and *b d* to the right, and if we unite them by converging the axes beyond the card, they will form in that position a resultant line in a *perspective* attitude receding as it extends downwards. Inverting the diagram but retaining it on the upper stage, we may by a like effort of convergence obtain a perspective resultant corresponding with the other in place and length, but having the reverse attitude, that is receding in the upward direction.

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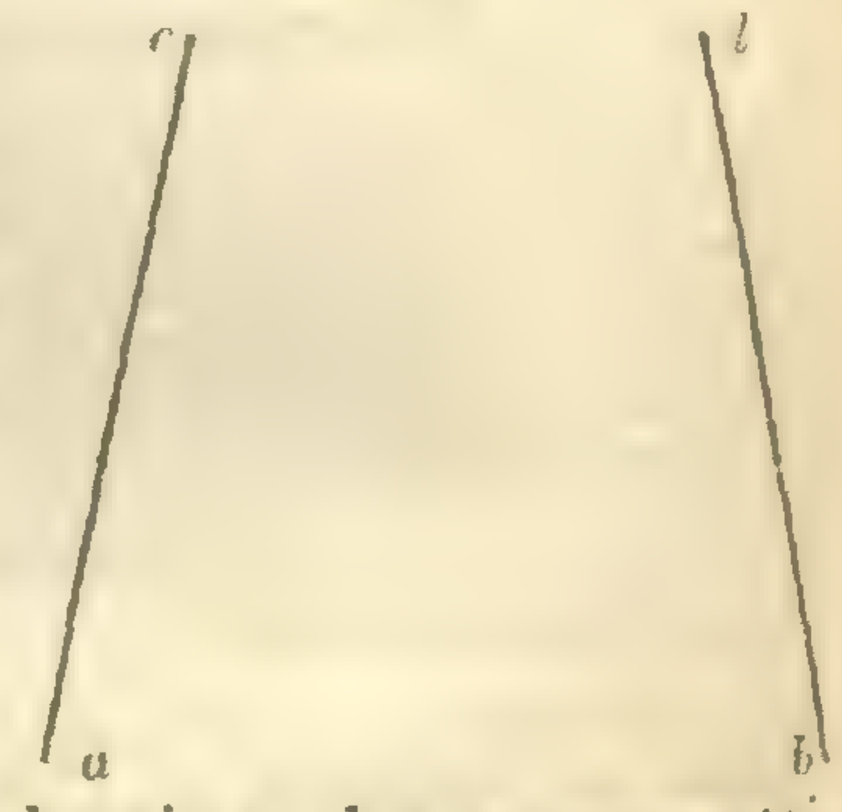

If now we place the card on the lower stage and combine the lines *a c* . . *b d* by cross vision in front of the figure, we obtain a perspective resultant which when the component lines converge upwards recedes in the upward direction, and when these lines are reversed recedes as it extends downwards.

It is scarcely necessary to remark that in all these experiments the *perspective attitude* of the resultant is a simple consequence of the unequal intervals between the corresponding parts of the component lines, requiring various degrees of optical convergence to effect their union. Thus in the first experiment the upper end of the resultant, formed by the union of *a* and *c*, will be situated behind the diagram at the point of convergence of the visual lines passing through these points from the left and right eye respectively, and the lower end of the resultant will be placed at the

point of convergence of similar lines, extended through *b* and *d*. Hence the lower end of the resultant will be at a greater distance than the upper end; and for a similar reason, the intermediate pairs of points in the component lines will be united at various intermediate distances, and thus the resultant will appear as a perspective line receding as it extends downwards. A like explanation obviously applies to the other cases above described.

Such is a general account of the formation of the perspective resultant by the union of inclined lines. But there are features in this phenomenon which from their bearing on the theory of binocular combination are deserving of a more minute and varied examination. As a first step towards this the following experiments are of interest as showing the chief conditions in the formation of the perspective resultant and illustrating the different degrees of completeness of the effect.

12. *Comparative results with lines differently inclined.*

Diagrams well suited for this comparison are represented in figs. 20, 21, and 22, in each of which the lines are separated below by an interval of $1\frac{1}{4}$ inch and have a vertical height of the same amount. The inclination of the lines in fig. 20 is 40°, and in the others 30°, 20°, and 10° respectively. Confining my remarks in the first place to the effects observed when the upper stage of the stereoscope is used and the binocular union is produced beyond the diagram, I begin with fig. 20, the lines of which make an angle of 40°.

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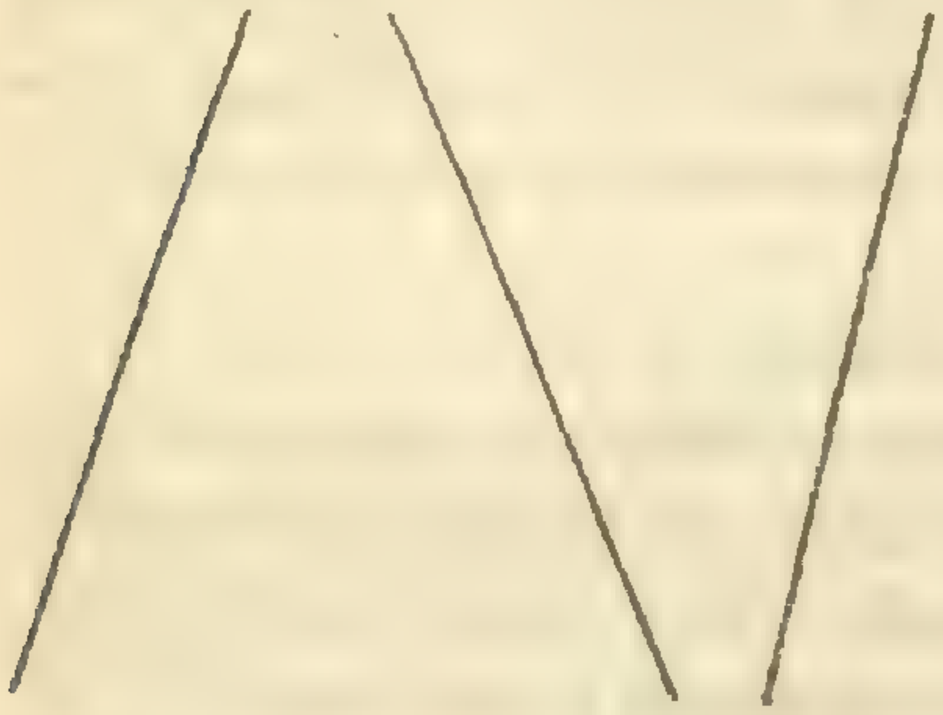

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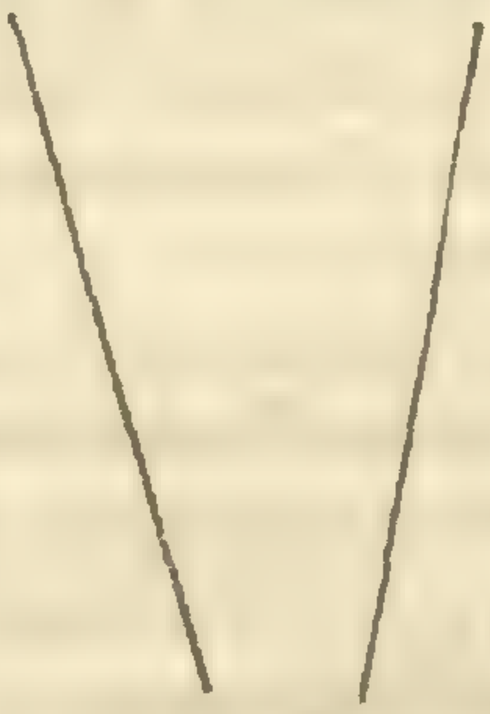


22.

Having placed the figure properly on the stage, I first adjust the visual convergence so as to bring together the upper ends of the lines, and maintaining this convergence, steadily I see the two lines combined into the figure of an inverted $\vee$. I then by a change of convergence unite the two lower ends and retaining the adjustment I observe them to form a $\vee$ in the erect position. In the same way by intermediate degrees of convergence I can cause the lines to appear as crossing one another at the middle or any other point of their length. But in either of these phases of combination it is to be noted, that *so long as the inclination of the optic axes proper to it is maintained without change, the resulting figure presents no appearance of relief*, but contin-

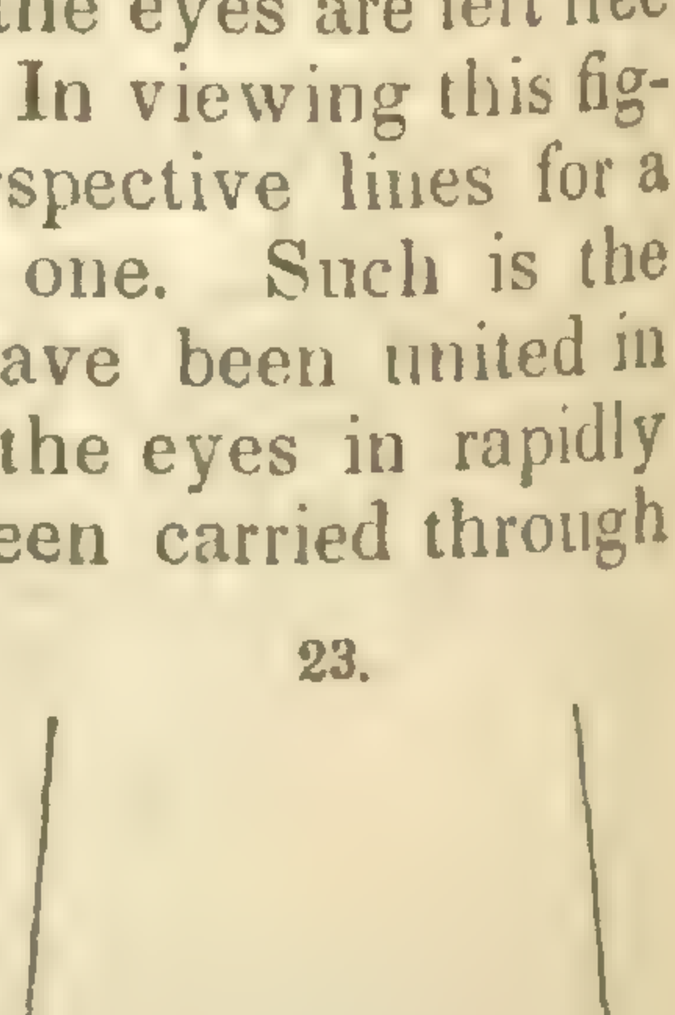
ues in the plane of the paper, or more properly in a parallel plane behind that in which the diagram is actually placed.

If now by suffering the eyes to range up and down along the resultant I combine the corresponding points in somewhat quick succession, I observe the various phases above mentioned to follow one another rapidly and at the same time I see the component lines assuming in part or wholly, the appearance of intersecting lines *in relief*. This effect, however, recurs only *transiently*, and as we observe it, the lines again subside into the plane parallel to the diagram. Owing to the great inclination of the lines in this figure, it is difficult to vary the convergence of the lines fast enough to unite the corresponding points in such quick succession as to bring about the perspective effect.

This is more readily done with fig. 21, whose lines are inclined at an angle of 30°. On first viewing these lines placed on the upper stage, I sometimes see a resultant consisting of intersecting lines without relief, and sometimes one consisting of intersecting perspective lines which on fixing the optic axes more steadily subside to the parallel plane. It is easy by giving the eyes free range along the resultant to continue this perspective effect, and again by compelling them to a slow change of convergence, to produce all the phases of intersection in the parallel plane without any appearance of relief.

Fig. 22 viewed under the same conditions, almost always presents the appearance of intersecting perspective lines, unless a special effort is made to preserve the optic axes in a fixed direction. But in this, as in the preceding case, such effect never fails to destroy the relief, which however, returns when the eyes are left free to their natural fluctuations of convergence. In viewing this figure it occasionally happens that the two perspective lines for a part of their length appear to be melted into one. Such is the result whenever their corresponding points have been united in very quick succession, in other words, when the eyes in rapidly glancing along a part of the figure have been carried through the necessary gradations of convergence.

23.



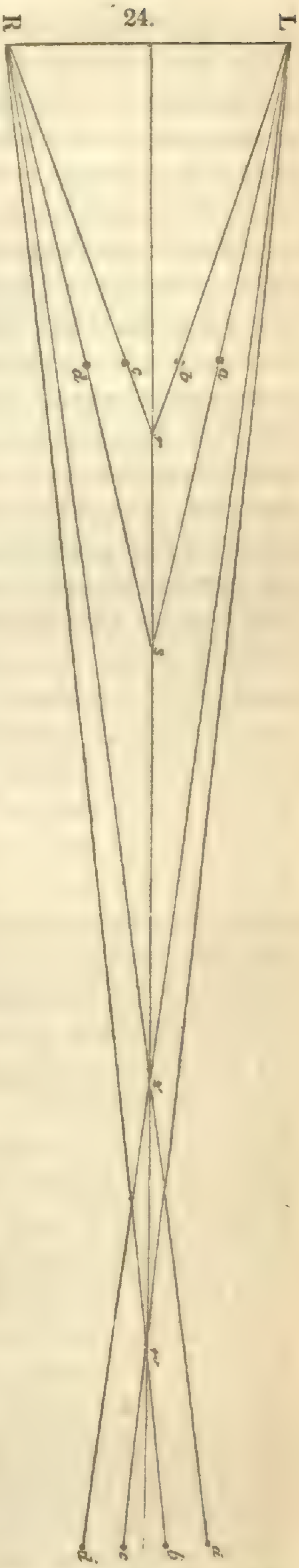
With fig. 23, the lines of which have an inclination of only 10°, the appearance of the resultant is sometimes that of a single perspective line and sometimes that of two such lines intersecting but very close to one another. It is important to remark that the latter effect always presents itself when we endeavor to fix the convergency of the axes, and that when we succeed in maintaining this direction, even for a few moments, the perspective lines, as in the preceding cases, lose their relief and present themselves in some phase of intersection in the parallel plane.

Owing to the small inclination of the lines in figure 23, a very slight fluctuation of convergence is sufficient to effect the successive union of many corresponding points, and this taking place involuntarily and almost instantaneously, gives us the perspective single or double resultant as the first or natural binocular effect. The effort necessary to prevent this slight and spontaneous fluctuation of convergence is such that unpractised observers find it difficult to bring the intersecting lines into the parallel plane. On first view they are apt to regard the result as a single perspective line, and it is only through a close and more deliberate inspection that they discover the two nearly coalescing intersecting lines of which the perspective figure consists. It is however, easy after some experience in these experiments, to maintain such perfect fixity of convergence at different stages of the binocular combination, as to give the resultant the form of intersecting lines in the parallel plane.

A similar gradation of effects is presented when the figures just referred to are placed on the lower stage of the stereoscope in the same order as before, but with this general difference that each corresponds in result to lines of less inclination on the upper stage. Thus I find that fig. 20 combined by cross vision in front of the lower stage, presents appearances like those above described in fig. 21; and so fig. 21 gives the perspective lines proper to fig. 22 on the upper stage, while this latter on the lower stage gives the almost single perspective line of 23 in the upper. In this comparison the upper stage is supposed to be 15 inches and the lower 31 inches from the rear end of the instrument.

This different grade of effect with the same figures, according as the lines are combined in front or behind their actual position, is evidently dependent on the much greater range of axial rotation required in the former than in the latter case in order to combine successively given parts or the whole of the inclined lines. Thus in fig. 24, if L and R denote the centres of the left and right eyes respectively, $a\ d$ the lower and $b\ c$ the upper ends of the lines placed on the upper stage, and $a'\ d'$ and $b'\ c'$ the same points of the figure as placed on the lower stage, it is obvious that a and d will be binocularly united at s and b and c at r; also that a' and d' will be combined at s' and b' and c' at r'. In passing from the union of a and d to that of b and c, that is in combining successively all the corresponding points of the two lines, the optic axes must be turned through the angles $a\ \mathrm{L}\ b$ and $d\ \mathrm{L}\ c$, respectively, making a total change of convergence equal to $2\,a\ \mathrm{L}\ b$. In combining successively all parts of the same lines placed at $a'\ d'$ the whole change of convergence is $2\,a'\ \mathrm{L}\ b'$, an angle less than the preceding nearly in the ratio of the distances of the lower and upper stages from the centre of the eyes. This comparatively small variation of convergence in the case of a given

pair of inclined lines on the lower stage brings them into the same relation to the converging movement of the axes as a pair of lines less inclined to one another placed on the upper stage.



From a comparison of the results above described we may I think, draw the following conclusions:

First. The eyes are disposed naturally or from habit to incessant slight changes of convergence which are effected unconsciously and with great celerity. Hence the continuance of any given state of convergence implies a constrained state of vision and requires a special effort of will.

Second. Great variations of convergence require a sensible and even considerable time, and are not spontaneous, but the result of voluntary effort.

Third. The successive union of corresponding points belonging to lines slightly inclined to one another, is effected almost instantaneously and without conscious effort. That of such points in lines having a much greater inclination is effected slowly and by a voluntary exertion.

Fourth. In the binocular combination of two mutually inclined lines no perspective effect is produced so long as the convergence of the optical axes is kept unchanged.

Fifth. The apparent relief or perspectiveness of the resultant presents itself only where contiguous corresponding points of the two lines are combined in rapid succession. Hence it never occurs when the lines are greatly inclined to one another, and it always shows itself in greater or less extent when their inclination is so small as to require an inconsiderable change of convergence.

Sixth. This perspectiveness is seen as well when the resultant is obviously composed of two intersecting lines, as when it appears as a single line, and it is destroyed whenever by voluntary exertion we suspend the usual or spontaneous changes of convergence.

In referring the perspectiveness of the resultant line or lines to the rapidly successive union of corresponding points, I would not

be understood as assuming that all the points of the component lines must be thus successively combined. It will hereafter, I think, be shown that this is neither necessary nor of usual occurrence. But I wish to give prominence to the fact that such a union of corresponding points through some portion perhaps or very small part of the two lines is requisite to the formation of a perspective resultant. This view differing almost equally from that of Sir D. Brewster and Prof. Wheatstone, will be considered in more detail under a future head.

The involuntary and rapid changes of axial convergence within certain small limits, which as we have seen, forms an interesting feature in such observations, does not seem hitherto to have attracted special attention; and the fact that *the resultant has a perspective position even when the lines forming it cross one another at a considerable angle*, has not I believe been referred to by previous observers, although as will hereafter appear, it has an important bearing on the theory of binocular perpective.

13. *Review of some experiments of Prof. Wheatstone bearing on the doctrine of successive vision.*

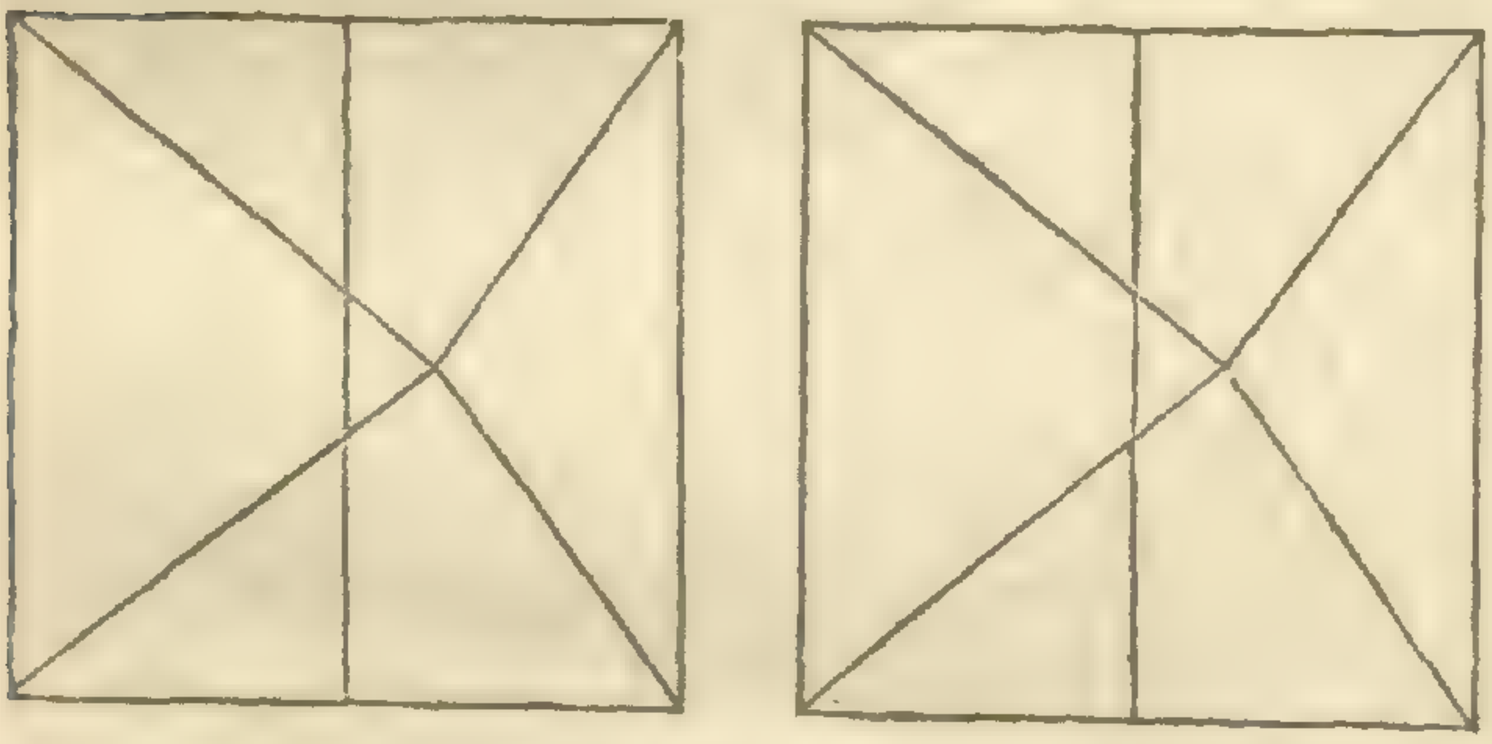
In commenting upon the doctrine of successive vision by points, Prof. Wheatstone remarks, that were this entirely true "no appearance of relief should present itself when the eyes remain intently fixed on one point of a binocular image in the stereoscope. But in performing the experiment carefully, it will be found that provided the pictures do not extend too far beyond the centres of distinct vision that the image is still seen single and in relief when this condition is fulfilled," (Phil. Trans.: 1838). The result here described is obviously at variance with the statements above made in regard to the resultant of two inclined lines viewed under a fixed convergence of the axes. But without in the slightest degree doubting the general accuracy and the great ingenuity of Prof. Wheatstone's observations, I am compelled in the present instance to dissent from his description of the phenomena. In multiplied trials with his reflecting stereoscope and the refracting one of Sir D. Brewster, as well as with my own apparatus, in the course of which I have submitted to examination most of the linear diagrams commonly used, I have invariably found that by converging the axes steadily on one point of the image the lines situated laterally near but behind or before it in the perspective, are made to appear *double*, and that by continuing the same fixed convergence these lines are made to *lose their relief* and to take their places in a plane at the same distance as the point to which the eyes are directed.

These effects are most striking in the case of figures having considerable depth of perspective and in which the lines and angles of the posterior surface are nearly covered by those of the anterior, as in binocular drawings of polyhedral crystals. Con-

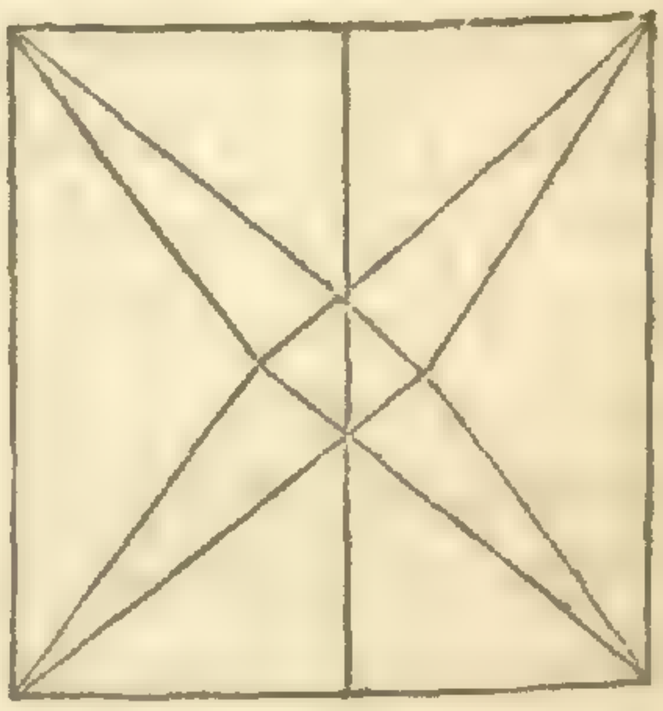
verging my eyes fixedly to the near apex of such a figure I see the remote one double and the oblique edges diverging from it also double; and in the same manner I find it easy to double every one of the angles or edges by fixing the eyes on the angle or edge lying in the same direction of view on the opposite side of the figure. When the image is a truncated square pyramid with its plane of truncation nearest the eyes, by looking intently on one of the vertical sides of the base I see the corresponding side of the smaller square double, or by fixing my eyes upon the latter I see the former double. In any of these cases a continuance of the same convergence causes the angles and lines in question to appear in the same plane.

Among the figures suited to illustrate this effect I have found none to present more striking results than the square pyramid or the truncated cone, with the addition in each case of a vertical line drawn through the centre of the base. The twin drawings adapted to produce these forms are represented in figs. 25 and 26.

25.



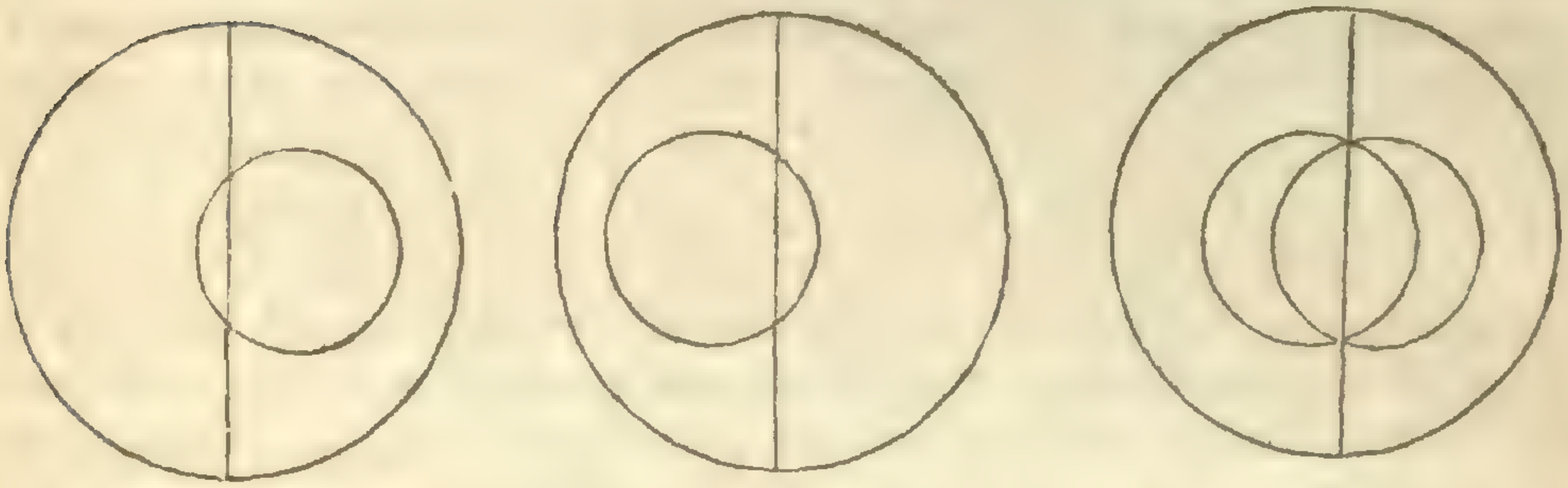
25*a*.



If, after obtaining the perspective resultant of fig. 25, I look intently on the apex of the pyramid so as to see it at a single point, I at the same time perceive the transverse line in the base as two parallel lines, and if I direct my view to the base so as to see this line as single, the apex is doubled and each of the oblique edges of the pyramid is converted into two lines diverging from an angle of the base. By continuing the same degree of convergence the image loses its relief and presents the appearance of fig. 25*a*, in which all the lines lie in one plane. Here the convergence being just adapted to the union of the two squares produces an effect equivalent to the superposition of the one plane figure on the other.

In the case of the truncated cone (fig. 26), on directing the axes to the vertical line in the base so as to see it distinctly single, the small end appears as two intersecting circles; fixing the view on the centre of the small end the vertical line in the base becomes double, and the large circle appears indistinctly as two intersecting circles. In either case a fixed convergence destroys

26. 26a.



the relief of the image. When this is done by fixing the view on the base, the resultant is the plane figure represented in fig. 26*a*, in the other case the figure is the converse of this.

It may be added than among the linear drawings commonly used with the stereoscope there are some in which these effects are less observable than in the above instances, and others in which the attempt to obtain a clear view of either the nearer or remoter lines of the perspective is attended with a painful degree of confusion. As this arises from the doubling of points or lines situated before or behind the point to which the axes are converged, it may be laid down as a rule in the selection of such figures for the stereoscope that *the most satisfactory drawings are those in which there is no anterior line crossing a posterior one and in which when two such lines are parallel they are not situated in or near the same vertical plane of view.*

As a very different degree of convergence is required for uniting the lines proper to the near side of the perspective image from that necessary for the remote side, more or less time must be consumed in passing from the one to the other. Hence in many cases even the most practised observer is unable to make this transition with sufficient celerity to have a clear perspective impression of the whole, and to this cause is no doubt due in part, the difficulty experienced by most persons unused to the stereoscope, in embracing distinctly the form and relief of the entire image.

Prof. Wheatstone cites "as equally decisive against this theory" of successive vision, the experiment of combining two inclined lines by converging the axes to a point nearer than the paper (Phil. Trans., 1838, or Phil. Mag., 1852). He says, "look intently on the upper end of the resultant line without allowing the eyes to wander from it for a moment. The entire wire will appear single and in its proper relief, and a pin or a piece of straight wire may without the least difficulty be made to coincide exactly in position with it." Of the precise visible coincidence between the resultant figure and the wire properly placed in this experiment there can be no question; but the important feature as I conceive in this phenomena is, that *it is only under particular*

conditions, and then only approximately, that either the image or the wire is seen as a single line.

In order to repeat the experiment under various conditions, I use the following arrangement. Fixing a strong sewing needle midway in a small cylinder of cork and firmly fastening into the cork one end of a thin stiff wire or knitting needle, so that the two needles shall be at right angles, I secure the former immovably in a horizontal position across the opening of the upper stage of my stereoscope very near the inner edge of the opening. I can then adjust the long wire to any inclination, and the friction of the cork on the horizontal needle will retain the wire in its position.

Taking fig. 21 or 22 as the subject of experiment, I place the drawing on the farther stage and move the other back or forward until a notch in the near end of the wire is brought to coincide precisely with the point of binocular union of the two lower ends of the component lines. I now unite the two upper ends of the lines by a due change of convergence, and depress the free extremity of the wire until the resultant point just mentioned coincides with this end or with some other point of the wire. When this is done, the wire is adjusted to coincidence with the perspective image. By sliding the stage a little nearer, we may see the image just behind the wire, and by moving it the opposite way it will appear on the near side of the wire. Whether coincident or in either of these positions, *both the image and the wire will appear as two perspective lines intersecting at the point on which the eyes are for the moment fixed.*

By continuing the same convergence, we cause the wire as well as the coincident image to subside from the perspective position, and we see them each resolved into two intersecting lines in a plane parallel to that of the diagram. Fixing the eyes on the near end of the wire or of the binocular resultant, they each appear in the plane in the form of a $\vee$; gazing intently on the remote end they present themselves as a $\vee$ inverted. If we direct our eyes intently on the plane of the paper we see the wire as two black wires covering or adjoining the inclined lines of the diagram.

With lines less inclined, as in fig. 23, we obtain similar effects; but as might be inferred from what precedes, whether we regard the wire or the coincident image, the divergency of the intersecting perspective lines is less remarkable than in the preceding cases, and a more perfect and continued fixity of axial direction is requisite to destroy their relief.

A very interesting effect is observed when the diagram is so placed on the farther stage that the wire after being adjusted as above, leaves uncovered a portion of each of the inclined lines. In this case the perspective figure is composed partly of the re-

sultant of the uncovered portions and partly of the wire, *the former appearing as the continuation of the latter.*

We may I think conclude from these experiments that the coincidence of the wire with the resultant of the inclined lines, instead of proving this resultant to be impressed on the eyes as a single line, merely shows that *in respect to relief* both it and the wire are viewed under the same visual conditions.

It is to be remarked that for these experiments the lines of the diagram ought to be as slender as is consistent with great distinctness and the wire as thin as it can be made without a loss of rigidity. When the lines are broad or the wire thick there is an appearance of singleness near the point of intersection due to the overlapping of the two images which in the case of slightly inclined lines may extend through much or all of their length.

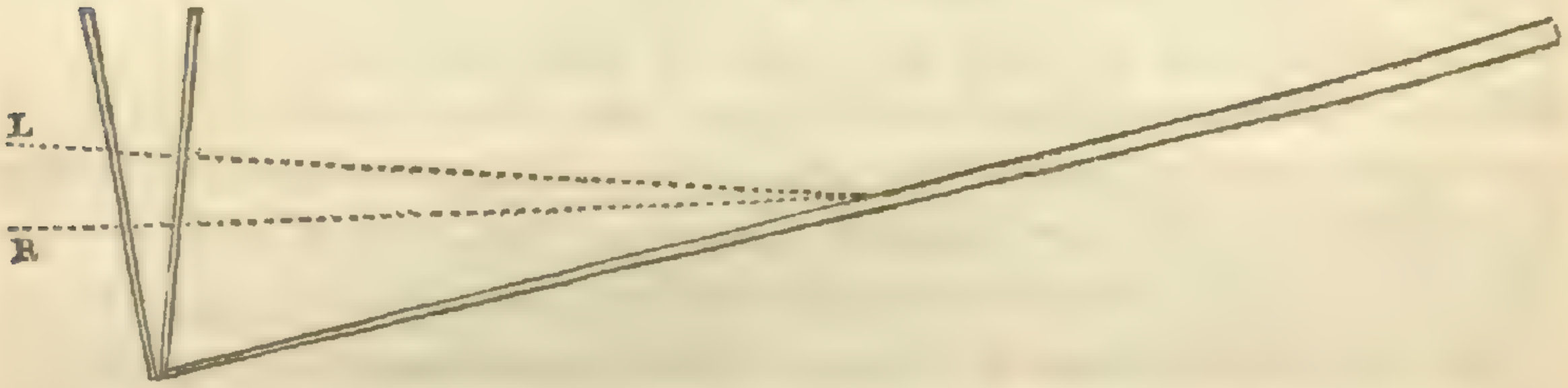
14. *Tapering form of the perspective resultant.*

In repeating these experiments it will be observed that while the wire coinciding with the perspective resultant appears of unchanging thickness throughout its whole length, this resultant seems to increase slightly in diameter from the near to the remote end. The cause of this difference although obvious, is not unworthy of remark. In the case of the wire the retinal picture in each eye is of course wider for the near than for the remote end, and would if uncorrected, convey the impression of an object tapering towards the farther extremity. But the different distances at which through varying convergence and other adjustments we see the different parts of the wire, serve to rectify this impression, and under the perception we obtain that of an object of uniform thickness throughout. In the case of the perspective image, on the other hand, the retinal picture in each eye is of uniform breadth, because the lines of the diagram are so and are practically in every part of their length at the same distance from the eyes. Thus the diameter of the resultant disc or lines actually subtends the same angle in both eyes for every part of the length. But in virtue of its perpective position, its different parts seem to be placed at different distances. Hence the diameter of any part of the resultant must appear greater in proportion to the distance at which it seems to be placed, and it will appear to grow broader as it is traced from the near to the remote end.

The effect here described is scarcely appreciable with most of the stereoscopic drawings, owing to the small range of distance through which the perspective extends. By using two inclined lines each three or four inches long and combining them when placed on the upper stage of the stereoscope I obtain a resultant whose remote end is more than twice as distant as the near one, and in this case the enlargement towards the farther extremity is very obvious.

A simple mode of obtaining the same effect in a yet more manifest degree is the following. Adjust two slender cylindrical rods six or seven inches long, so that, while they nearly touch at one end, they spread apart about an inch at the other. Hold them before the eyes R, L, at the distance of distinct vision in a vertical plane transverse to the line of view and with the angle downward (fig. 26). Then form the binocular resultant by directing

27.



the axes beyond them with varying convergence. This resultant stretching away from the angle obliquely towards the wall will be seen to increase in diameter regularly from its near to its remote end.

From these facts it may be inferred that in order with the stereoscope to obtain a perspective figure, all the lines of which shall be of precisely equal thickness compared with one another, and each of them of the same thickness at every point, it would be necessary so to construct the drawings that the breadth of the corresponding parts of the component lines should be proportioned inversely to the distances at which they are to be united.

(*To be continued.*)

ART. XXI.—*On the identity of Sanguinarine and Chelerithrine, and on the direct determination of Nitrogen;* by Dr. JAMES SCHIEL of St. Louis, Mo.

I HAVE used a small portion of hydrochlorate of Chelerithrine which I received in 1843 from my friend, Prof. H. Will of Giessen, and which the discoverer of that alcaloid had prepared himself, to make a few analytical experiments as to its composition.

As the salt was not found to be free from impurities, it was dissolved in water, precipitated by ammonia, washed, dried, dissolved in ether, the filtered solution treated with animal charcoal, and the chelerithrine precipitated with a solution of pure sulphuric acid in ether; the sulphate of chelerithrine was washed with ether, dried and dissolved in water, when it yielded with ammonia a precipitate of pure Chelerithrine, which absolutely showed the same properties and behavior as Sanguinarine.

Of the pure substance dried at 105° C.,

0·356 grammes burned with oxyd of copper and oxygen, furnished 0·918 carbonic acid = 70·34 per ct. of carbon and 0·167 water = 5·21 per ct. of hydrogen.

The direct determination of nitrogen gave 5·07 per ct.

0·3925 gramm. of the double salt of the hydrochlorate of chelerithrine and chlorid of platinum yielded 0·707 of platinum: thence the atomic weight = 341·74.

Accordingly the composition of Chelerithrine is:

Carbon, - - - -	70·34
Hydrogen, - - - -	5·21
Nitrogen, - - - -	5·07
Oxygen, - - - -	19·38
	100·00

The composition of Sanguinarine I found:*

Carbon,	70·03	69·82	70·02
Hydrogen,	5·27	5·08	5·14
Nitrogen,	5·23		
Oxygen,	19·14		
	100·00		

The atomic weight was found in three determinations 322·7; 362·7; 346·4, or taking the mean =343·9.

From all this it appears that Chelerithrine and Sanguinarine are one and the same substance corresponding to the formula $C_{38}H_{16}NO_8$. This formula gives:

Carbon, - - - -	70·80
Hydrogen, - - -	4·97
Nitrogen, - - - -	4·35
Oxygen, - - - -	19·88
	100·00

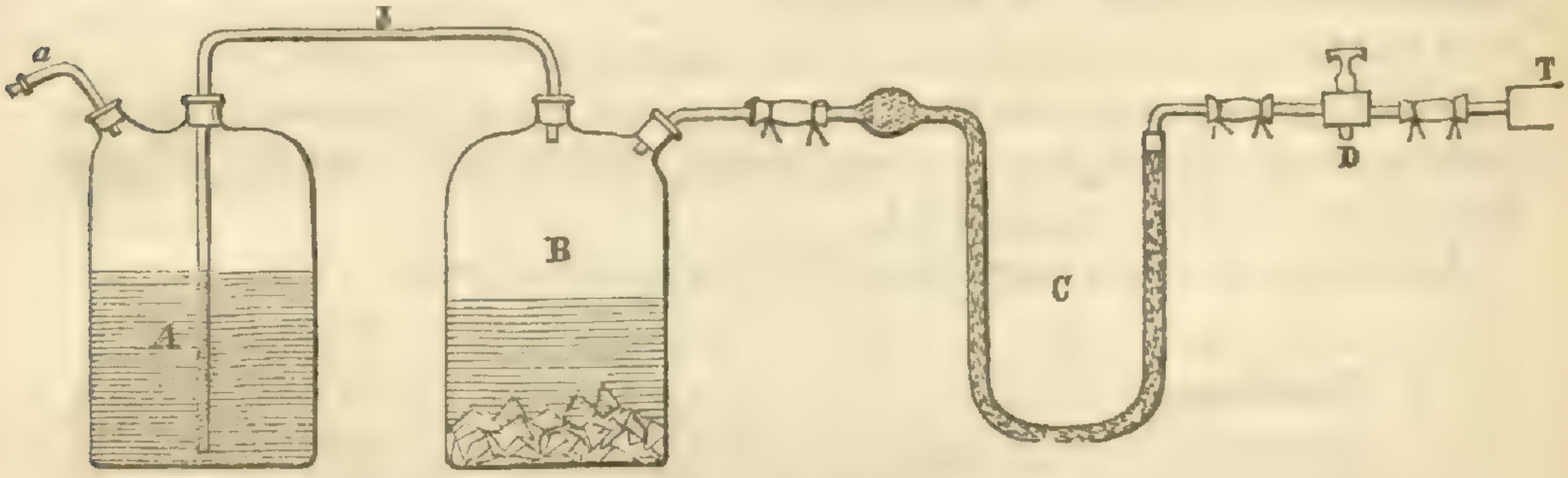
It will be perceived that the above formula includes one equivalent of carbon more than the one I formerly deduced from the analysis of Sanguinarine.

For the preparation of one or the other of these alcaloids the following method is the simplest and cheapest.

Digest the root of the plant Sanguinaria Canadensis (or Chelidonium majus) with water strongly acidulated with sulphuric acid and precipitate with ammonia, wash and dry the precipitate, dissolve in ether, and treat with animal charcoal. After filtration the alcaloid is precipitated with a solution of pure sulphuric acid in ether. It is pure sulphate of sanguinarine.

* Annalen d. Chemie und Pharmacie by Liebig and Wöhler, B. XLIII, p. 233.

For the determination of nitrogen I do not use the bicarbonate of soda but the following apparatus, from which a current of carbonic acid is passed through the combustion tube.



A is a Woulfe's bottle containing diluted muriatic acid.
B, a similar bottle with pieces of chalk and water.
C, Chlorid of calcium tube, part of it filled with chalk; a piece or two of pumice stone separates the two substances.
T, Combustion tube.

Condensing the air in bottle A by blowing through the mouth-piece *a*, some of the acid passes into B and sets carbonic acid free, which passing through A washes the atmospheric air contained in B mostly out, then close the mouth-piece with a cork and open the little stop-cock D. When a part of the gas passing through the combustion tube is entirely absorbed by the chalk, the stop-cock D is closed, the cork taken out from the mouth-piece, and the combustion is performed in the common way. After the combustion, another current is passed through the combustion tube by opening the stop-cock and closing the mouth-piece, having previously driven some acid into B. In case of need the stop-cock could be omitted, but the little expense hardly justifies such a simplification.

ART. XXII.—*On an apparent Perturbation of the Law of Definite Proportions observed in the Compounds of Zinc and Antimony*; by JOSIAH P. COOKE, Jr., Cambridge.*—With a plate.

IN a former paper in this Journal† I described two new compounds of zinc and antimony $Sb\,Zn_3$ and $Sb\,Zn_2$ which I named respectively, Stibiotrizincyle and Stibiobizincyle, because they resemble in their composition the metallic radicals of organic chemistry, and because the first decomposes water rapidly at 100° C. I there stated that crystals of $Sb\,Zn_3$ could be obtained containing a much larger amount of zinc than that required by the law

* Abstract from a Memoir of the American Academy, New Series, vol. v, p. 337.
† This Journal, vol. xviii, p. 234.

of definite proportions, and that this change was not accompanied by any alteration of crystalline form. A similar variation of composition was afterwards observed in the crystals of $SbZn_2$, and it is the object of the present paper to describe the law of the variation in both cases and to explain its cause.

In the course of my investigations on this subject, crystallizations were made or attempted of alloys differing in composition by one half to five per cent., according to circumstances, from the alloy containing 95 per cent. of zinc to that containing 95 per cent. of antimony; but only two crystalline forms were observed, that of $SbZn_3$ and that of $SbZn_2$. Well defined crystals, like those described under $SbZn_2$ in the former paper,* were obtained from the alloys between 43 and 60 per cent. of zinc; and even in alloys of a higher zinc per-centage, crystals of the same form were still seen, although they were no longer well defined. In the alloys between 20 and 33 per cent. of zinc, well defined crystals, like those described under $SbZn_2$ in the same paper, were formed; and finally there separated from the alloys between 33 and 42 per cent. of zinc thin metallic plates, which evidently belonged to the same crystalline form. In making the alloys from 43 to 95 per cent. of zinc, the zinc was melted first, and when in fusion the antimony was added. As the melting point of antimony is much above that of zinc, the fluid zinc acted on the solid antimony as a solvent, dissolving the pure metal, but not the impurities, which rose to the surface forming a scum. This scum seemed to take with it some of the antimony and thus caused a loss, which, together with the impurity, was found by experiment to be about three per cent. of the antimony used. This resulted in raising the per-centage of zinc in the alloy at most about eight-tenths of one per cent. The alloys below 43 per cent. of zinc were made by melting the antimony first, and then adding zinc. By this method the loss of antimony was very greatly diminished, and, counting the impurity, was found to be only about one per cent. and a half of the antimony used. In preparing the alloys this loss was always allowed for, and the crystallizations were all made as nearly as possible under the same circumstances, so that any unexpected cause of error should affect all equally. The crystals formed in the alloys were all analyzed in my laboratory under my direction and immediate supervision, and the greater part of them by myself. The rest were by my assistants, Mr. F. H. Storer, Mr. C. W. Eliot, and Mr. C. S. Homer, to whose care and accuracy I take pleasure in bearing witness. Their work is in all respects as reliable as my own. The results are collected in the following table which will explain itself.

* This Journal, vol. xviii, p. 234.

Analyses of the Crystals formed in the Alloys of Zinc and Antimony.

STIBIOTRIZINCYLE.						STIBIOBIZINCYLE.					
Composition of the Alloys by Synthesis.		Composition of the Crystals by Analysis.			Name of the Analyst.	Composition of the Alloys by Synthesis.		Composition of the Crystals by Analysis.			Name of the Analyst.
Per ct. of Zn.	Per ct. of Sb.	Per ct. of Zn.	Per ct. of Sb.	Sum.		Per ct. of Zn.	Per ct. of Sb.	Per ct. of Zn.	Per ct. of Sb.	Sum.	
70·40	29 60	64·15	35·77	99·92	Cooke.	33·00	67 00	35·37	64·57	99·94	Cooke.
66·50	33·50	61·00	39·00	*100·00	Cooke.	do.	do.	35·40	34·60	†100·00	Cooke.
64·50	35·50	53 50	41·44	99·94	Cooke.	32·50	67·50	34·62	64·92	99·54	Storer
. .	. .	55·49	44·42	99·91	Homer.	do.	do.	34·61	65·39	†100·00	Eliot.
60·60	39·40	55·00	45·09	100·09	Homer.	31·50	68·50	33·95	66·09	100·04	Storer.
58·60	41·40	50·39	49·29	99·68	Eliot.	29·50	70·50	33·62	66·38	†100·00	Storer.
56·60	43·40	49·92	50·05	99·97	Eliot.	do.	do.	33·62	66·38	†100 00	Storer.
54·70	45·30	48·26	51·42	99·68	Storer.	27·50	72·50	33·85	65·81	99·66	Storer.
52·70	47·30	47·47	52·53	†100·00	Cooke.	26·50	73·50	32·08	67·60	99·68	Storer.
. .	. .	. .	. .	. .	. .	26·00	74·00	30·74	69·06	99·80	Cooke.
50·70	49·30	46·89	53·11	†100·00	Cooke.	25·50	74·50	30·43	69·51	99·94	Storer.
do.	do.	46·45	53·55	†100·00	Cooke.	25·00	75·00	29·88	70·20	100·08	Cooke.
48·70	51·30	48·66	51·34	†100·00	Eliot.	24·50	75 50	28·76	71·24	100·00	Cooke.
46·70	53·30	46·77	53·23	†100 00	Eliot.	23·50	76·50	27·93	71·85	99·78	Cooke.
44·80	55·20	44·26	55·73	†100·00	Eliot.	22·50	77·50	26·62	73·27	99 89	Storer.
43 80	56·20	44 04	55·96	†100·00	Cooke.	21·50	78 50	24·83	74·74	99·57	Cooke.
42·80	58·20	43·15	56·93	100·08	Cooke.	20·12	79·88	20·58	79·42	100·00	Cooke.
do.	do.	43·06	56·50	99·56	Cooke.						
do.	do.	42·83	57·24	100·07	Cooke.						

Curve of Variation in Composition.—In order to compare together the composition of the crystals and that of the alloy in which they form, I have resorted to the usual method of Analytical Geometry, and in the plate illustrating this paper, the lower horizontal line is the axis of abcissas, and the vertical line at the extreme left the axis of ordinates. The first has been divided into equal parts, which denote the per cents. of zinc in the crystals and the last into parts of the same size, which stand for the per cents. of zinc in the alloys. The zinc rather than the antimony determinations have been selected for comparison, as being generally more accurate, and as having been all made in exactly the same way. The points determined by analysis are indicated with dots, and the double line drawn through these dots is a curve, which represents the relation of the composition of the crystals to that of the alloy in which they form. In order to make clear the connection between the two, it will be well to discuss this curve, commencing with what may be termed the two centres of crystallization, the alloys of 42·8 and 31·5 per cents. of zinc, and examining the effect produced on the crystals by diminishing or increasing the amount of zinc in the alloy.

It has already been stated that the crystals of $Sb\,Zn_3$ are obtained in their greatest perfection from the alloy of 42·8 p. c. of zinc. They are then comparatively large, generally aggrega-

* In this analysis the antimony only was determined.
† In this analysis the zinc only was determined.

ted, and, as the three analyses cited in the former paper prove, have the same composition as the alloy. On increasing gradually the amount of zinc in the alloy up to 48·7, the crystals continued to have the composition of the alloy, and the only difference, which could be observed in their character, was, that they were smaller and more frequently isolated. Between these limits, the whole mass of the alloy exhibited a strong tendency to crystallize, and, by pouring it, as it cooled, from one vessel to another, it could be crystallized to the last drop. The portion *a*, *b*, of the curve is therefore a straight line equally inclined to the two axes. On increasing the amount of zinc in the alloy to 50·7 p. c., the amount of zinc found in the crystals was only 46·89 p. c., and above this it was uniformly less than it was in the alloy; but no closer relation between the two could be detected, owing undoubtedly to the unavoidable irregularity in the crystallizations of the alloys, which contained more than 50 p. c. of zinc. This arose from a peculiar pasty condition, which the fluid mass assumed, at the point of crystallization, apparently caused by the separation of the excess of zinc. Definite crystals however were obtained even from the alloy of 60 p. c. of zinc, which contained 55 p. c.; above this, the crystals became less and less abundant, and gradually faded out, although the alloy even of 86 p. c. of zinc exhibited a radiated crystalline texture; and a trace of this structure could still be discovered even in the alloy containing only 4 p. c. of antimony. It might be supposed that on returning to the alloy of 42·8 p. c. of zinc, and increasing the amount of antimony we should obtain crystals containing an excess of antimony; but so far is this from being true that the slightest excess of antimony entirely changes the character of the crystallization. On crystallizing an alloy containing 41·8 p. c. of zinc not a trace of any prismatic crystals could be seen, but in their place there was found a confused mass of thin metallic scales, which, as will soon be shown, are imperfect crystals of $Sb\,Zn_2$. Thus it appears that although perfectly formed crystals of $Sb\,Zn_3$ can be obtained containing 55 p. c. of zinc they can not be made to take up the slightest excess of antimony.

In order to obtain crystals having the composition of $Sb\,Zn_2$, that is, containing 33·5 p. c. of zinc, it is necessary to crystallize an alloy at least as low as 31·5 p. c. of zinc. At this point large compound crystals are obtained corresponding to the large crystals of $Sb\,Zn_3$. On increasing the amount of zinc in the alloy up to 33 p. c., the proportion of zinc in the crystals appeared to increase in the same ratio, so that the curve of $Sb\,Zn_2$ is at this part a straight line parallel to the curve of $Sb\,Zn_3$. It should however be noticed that the extent of this line *k i* is so limited that a very small error in the analyses might change very considerably its direction. The crystals of $Sb\,Zn_2$ containing an excess of zinc,

are smaller and more frequently isolated than those containing exactly two equivalents. A similar fact, it will be remembered, is true of the crystals of $SbZn_3$. At the alloy of 33 p. c. of zinc, the definite crystals of $SbZn_2$ begin to disappear and are succeeded by thin metallic scales, which, as the two following facts will prove, are imperfect crystals of the same crystalline form. First, the scales from the alloy of 33 p. c. are frequently found having a definite crystal as a nucleus, when it is evident that their surfaces are extensions of the basal plane O of fig. 2 of the former memoir. Secondly, the scales twin together like the large tabular crystals of $SbZn_2$, forming a cellular structure, and the angle between two scales thus united, measured with an application goniometer approximately 115° 30′, and was therefore equal to the basal angle of the definite crystals. These scales continue up to the alloy of 41·8 p. c. of zinc, becoming however constantly less abundant and less distinct. Several specimens of them were analyzed, but no regularity in their composition could be detected except that they all contained a very much larger amount of zinc than the alloys in which they formed. This irregularity and the imperfection in the crystallization seem to be caused by the interference of $SbZn_3$, that is, by a tendency to form $SbZn_3$ which exhibits itself in a proneness of the crystals of $SbZn_2$ to an excess of zinc. The line *k i* has been continued with dots in order to show that the influence of $SbZn_2$ extends as far as the alloy of 42·8 p. c. of zinc. On returning to the alloy of 31·5 p. c. of zinc and adding an excess of antimony it was found that the crystals formed continued to have the theoretical composition of $SbZn_2$ until the amount of zinc in the alloy had fallen to 27 p. c., so that the tendency towards the theoretical composition was so great, that in the alloys between 31·5 and 27 p. c. of zinc, crystals were formed having very nearly this composition. On still further increasing the amount of antimony in the alloy, the composition of the crystals gradually approached that of the alloy, and from the alloy of 20·2 p. c. of zinc, very imperfect crystals were obtained having almost the same composition as the menstruum. At the same time, the crystals became less and less perfect and finally disappeared altogether in the alloys below 20 p. c. of zinc.

The portion of the curve *k m n h*, is the most important result of this investigation and therefore deserves especial notice. It has been shown that crystals of the form of $SbZn_2$, or at least crystalline scales of the same character, are formed in the alloys between 20 and 43 p. c. of zinc, the first per cent. corresponding to Sb Zn and the second to $SbZn_3$. Half way between these two points, that is the alloy of 31·5 p. c., is the point where crystals having the calculated composition of $SbZn_2$ are first obtained. Were the variations in the composition of the crystals of $SbZn_2$ exactly proportioned to the excess of zinc or of anti-

mony in the alloy, as is the case with Sb Zn_3, then the curve of variation would be the straight line formed by the continuation of the line *a b*. From this line *b h* the course of the curve is deflected by the force which determines the union of the elements in definite proportions, and which for the want of a special term, I will call the Chemical Force. This is so strong that the curve runs parallel to the axis of ordinates through the distance *k m*. Beyond this point, the influence of the excess of antimony in the alloy becomes stronger than the chemical force, and the curve gradually bends towards the line *h b* which it finally meets at *h*. In the portion *h n* of the curve, the analyses are best represented by the arc of a circle, of which the radius equals *h e* or one-half of *h b*, and to which the line *k m* is tangent. In the portion *n m* the points determined by analysis may also be connected by the arc of a circle of which the radius *o' n* equals the difference between the radius *o n* and twice *g n*, so that the two centres are at the same distance from the line *a h*. The whole curve is evidently the result of two forces; one acting along the chord in the direction *b h*, a force tending to increase the amount of antimony in the crystals proportional to the amount in the alloy, the same force in fact, which acts undisturbed in forming the portion of the curve *b a*; the other the chemical force acting in the direction of the tangent *k m*. It has already been stated that crystals having the calculated composition of Sb Zn_2 are not first formed in the alloy of the same composition 33·5 p. c. of zinc, but in an alloy containing two per cent. less; so that the line *m k*, instead of extending to *e*, changes from this direction at *k*, and afterwards runs parallel to the line *b h*. Unless this fact can be explained by a tendency in Sb Zn_2 to an excess of zinc caused by the influence of Sb Zn_3 as suggested above, the reason of this difference between Sb Zn_2 and Sb Zn_3 in this respect is not clear; but as some evidence that it is not accidental it may be stated, that the distance *k c* equals *c i*, the last point being the one, at which the tangent line *m k* extended meets the curve. Another remarkable fact whose bearing cannot be at present seen, but which like the last serves to corroborate the general accuracy of the result was pointed out by my colleague, Prof. Peirce, after the plate had been engraved. The distances of the three most important points of the curve of Sb Zn_2 from the line *a h*, viz. *k d*, *m f*, and *n g*, are simple multiples of the first; *n g* is twice and *m f* three times *k d*. The curve has been fixed, as will be noticed from the dots, by a large number of points determined throughout the greater part of its length at every per cent., and in the portion *m n* at every half per cent.; they certainly coincide with the curve as closely as could possibly be expected, and the very agreement of so many different determinations by three separate analysts is a strong proof of the general correctness of the work.

By making hypotheses in regard to the nature of the two forces, which have generated the curve just described, it would not be difficult to obtain for it a mathematical expression; but as such hypotheses, in our ignorance of the nature of these forces, would be premature, I must content myself with giving its geometrical construction on a chart ruled like the plate illustrating the memoir. Let the coördinates of any point of the curve be, x = per cent. of zinc in the crystals, and z = per cent. of zinc in the alloy. In order to construct the curve of $Sb\,Zn_3$, find a point (a) of which $x = z = 43$ p. c. (the calculated per cent. of $Sb\,Zn_3$) and draw a straight line $a\,b$ equally inclined to the two axes in the direction from the origin. To construct the curve of $Sb\,Zn_2$, produce the line $a\,b$ in the opposite direction to the point $x = z = 20$, which will be the lowest point of the curve. Find next a point (k) of which $x = 33{\cdot}7$ p. c. (the calculated per cent. of $Sb\,Zn_2$ is $33{\cdot}5$) and $z = 31{\cdot}5$ p. c., which is one-half of $43 + 20$. Through this point draw a line $m\,k$ parallel to the axis of ordinates and intersecting the line $a\,b\,h$ at c. The line $m\,i$ is the tangent, and the line $b\,h$ the chord of the required arc. On the line $m\,i$ take $c\,i = c\,k$, and i is the point at which the arc should touch the tangent. Erect a perpendicular on the tangent at the point i, take $o\,i = \frac{1}{2}\,b\,h$, and from o as a centre, with a radius $= o\,i$, describe the arc $h\,i$. Also from the centre o let fall a perpendicular $o\,g$ on the chord $b\,h$, and produce it to a point o' making $o'\,g = o\,g$. It will intersect the arc at (n). From o' as a centre with a radius $o'\,n$ describe a second arc $n\,m$ intersecting the tangent at m. Finally, draw from k, a straight line $k\,l$, parallel to $b\,h$, then the broken line $l\,k\,m\,n\,h$ will be the required curve.

It will be noticed that the tangent which has been drawn on the plate through the points determined by analysis is two-tenths of a per cent. in advance of the line which would correspond to $Sb\,Zn_2$. This position is essential to the equality of $k\,c$ and $c\,i$, if we retain as the value of the radius of the larger arc $R = \frac{1}{2}\,b\,h$. If the analyses should have given erroneously too much zinc so that the true position of the line should be at $x = 33{\cdot}5$ per cent., then this equality would be destroyed, and the conditions for finding the centre o would be reduced to the coördinates of the point h, the length of the radius and the position of the tangent, from which by a very simple construction the curve might be drawn. It should however be remarked that the position of the tangent in advance of the line $x = 33{\cdot}5$ is in accordance with the fact, already noticed, that the crystals of $Sb\,Zn_2$ have throughout a proneness to an excess of zinc caused apparently by the influence of $Sb\,Zn_3$; but it is also true that the tendency of the error in the zinc determinations is in the same direction.

Before discussing the conclusions to which the facts already stated seem directly to point, it will be well to see how far the variation in composition corresponds to a variation in the properties of the two compounds. Three classes of properties have been examined in this connection, viz., Specific Gravity, Crystalline Form, and Affinity for Oxygen, which will be treated of in order.

Specific Gravity.—The specific gravities of all the crystals analyzed, as well as that of the zinc and antimony used in the investigation, were taken with the greatest care. The determinations were made with a nicely constructed specific gravity bottle, as this method was found susceptible of greater accuracy than any other, when the temperature was observed with precision. In calculating the specific gravity, the weight of the water was corrected for the temperature, so that the unit is in all cases distilled water at 4° C. A similar correction could not be made for the temperature of the substance, as the coefficients of expansion of the crystals are not known. The results of the determination all made by myself are collected in the following table in the column headed "Sp. Gr. by Experiment." In the column

Specific Gravities of Crystals, formed in the Alloys of Zinc and Antimony.

Composition of the Alloys.		Composition of the Crystals		Sp.Gr. of Crystals by Experiment.	Mean Sp. Gr. of Zinc and Antimony.	Expansion in Crystallizing.
Per ct. of Zn.	Per ct. of Sb.	Per ct. of Zn	Per ct. of Sb.			
100·00				7·153	7·153	0·000
*96·00	4·00			7·069	7·134	0·065
*86·20	13·80			6·898	7·086	0·188
*76·30	23·70			6·769	7·039	0·270
70·40	29·60	64·20	35·80	6·699	6·982	0·283
66·50	33·50	61·00	39·00	6·628	6·967	0·339
64·50	35·50	58·56	41·44	6·596	6·956	0·360
62·50	37·50	55·53	44·47	6·506	6·941	0·435
60·60	39·40	55·00	45·00	6·440	6·939	0·499
58·60	41·40	50·39	49·61	6·396	6·917	0·521
56·60	43·40	49·95	50·05	6·388	6·915	0·527
48·70	51·30	48·66	51·34	6·404	6·909	0·505
46·70	53·30	46·77	53·23	6·376	6·900	0·524
44·80	55·20	44·26	55·74	6·341	6·888	0·547
42·80	57·20	43·09	56·91	6·327	6·882	0·555
*40·00	60·00			6·386	6·867	0·481
*35·00	65·00			6·404	6·844	0·440
33·00	67·00	35·37	64·63	6·401	6·845	0·444
29·50	70·50	33·62	66·38	6·384	6·837	0·453
27·50	72·50	33·85	66·15	6·383	6·838	0·455
26·50	73·50	32·08	67·92	6·400	6·829	0·429
26·00	74·00	31·07	68·93	6·418	6·824	0·406
25·50	74·50	30·43	69·57	6·428	6·822	0·394
24·50	75·50	28·76	71·24	6·449	6·813	0·364
22·50	77·50	26·62	73·38	6·453	6·803	0·350
21·50	78·50	24·83	75·17	6·467	6·795	0·328
*15·00	85·00			6·564	6·748	0·184
*10·00	90·00			6·603	6·725	0·122
*5·00	95·00			6·655	6·701	0·046
	100·00			6·677	6·677	0·000

* Alloys not crystallized.

headed "Mean Sp. Gr. of Zinc and Antimony" are given the calculated specific gravities of the same crystals on the supposition that the two metals had undergone no expansion on uniting. The last column was obtained by subtracting the numbers of the former from those of the latter, and therefore shows the relative amount of expansion. On examining the table, it will be found 1st. That the union of antimony and zinc is accompanied by expansion. 2nd. That the specific gravity of the crystals varies slightly with the composition. 3d. That the two minimum specific gravities correspond precisely to the composition of $Sb\,Zn_2$ and $Sb\,Zn_3$, so that the specific gravity increases and the expansion diminishes as you depart on either side from these two centres. 4th. That the specific gravity of $Sb\,Zn_3$ is smaller than that of $Sb\,Zn_2$. We find then that the specific gravity determinations confirm in general the results of the analysis pointing out the same two centres of crystallization.

Crystalline Form.—It has already been stated that only two crystalline forms can be obtained from the alloys of zinc and antimony, that of $Sb\,Zn_3$ and that of $Sb\,Zn_2$. A large number of crystals of $Sb\,Zn_3$ from different alloys, and therefore containing different proportions of zinc, were carefully measured for the purpose of ascertaining whether the angle was at all affected by the variation of composition. Fortunately four different crystallizations afforded excellent crystals, the angles of which could be measured to a minute. The crystals contained respectively 43·15, 44·14, 46·90 and 55·00 per cents. of zinc, and on all these by repeated measurements the angles were found to be *identical* with those given under figs. 1 and 2. Crystals from many of the other alloys were also measured, but on account of the imperfections of their surfaces the angles could not be determined within five or ten minutes. In all these cases however the values of the angles given above were included within the limits of uncertainty.

The faces of the crystals of $Sb\,Zn_2$ are not generally so perfect as those of $Sb\,Zn_3$, nor is their tabular form so well adapted for measurement; moreover variations in some of the angles have been noticed in crystals from the same crystallization amounting even to ten minutes. The angle 0 on 1 however appeared to be very constant for in all cases where it could be accurately measured the same value was obtained. As none of the crystals of $Sb\,Zn_2$, containing an excess of antimony, could be measured with precision, no constant variation of angle could be detected and on the other hand it could not be proved to be invariable.

Affinity for Oxygen.—The affinity of the crystals of $Sb\,Zn_3$, of different compositions, for oxygen, may be estimated by comparing the amounts of hydrogen gas evolved in a given time on boiling alloys of the same composition with water. The results

of such experiments were given in the former memoir in a table, a mere glance at which will discover the two following facts—

1st, That up to 40 per cent no great increase in the amount of hydrogen evolved is obtained by increasing the amount of zinc in the alloy.

2nd, That at the alloy containing 42 per cent. of zinc there is an immense maximum confined at most between two per cent on either side.

General Conclusions.—Before stating the conclusions to which as I think the facts now established directly point, it will be well to consider the only two admitted principles of chemical science which could possibly be brought forward to explain similar variations. They are, first, that of impurities in crystals; second, that of isomorphous mixtures. It will not be difficult to show that the variations in composition of $SbZn_2$ and $SbZn_3$ cannot be caused by either of these principles.

It is a well known fact that crystals frequently take up impurities which are either dissolved or mechanically suspended in the menstruum in which they form, and it might be supposed at first sight that the excess of zinc or antimony in $SbZn_3$ or $SbZn_2$, bore the same relation to their crystals that the sand does to the rhombohedron of calcite from Fontainebleau, or oxyd of iron and chlorite to crystals of quartz; but, in the first place, in all cases where a considerable amount of impurity is present the crystals are either imperfect or else the angle is considerably changed at times even as much as two or three degrees; and secondly, as such impurities are merely mechanical, the amount in the crystals would in all probability be proportional to the amount present in the menstruum at the time of their formation. Now in the crystals of $SbZn_3$, from the alloy of 60 p. c. of zinc, there is present an excess of zinc amounting to 15 p. c. and nevertheless the crystals are as perfect as, and their angles identical with, those obtained from the alloy of 43 per cent. In the crystals of $SbZn_3$ the excess of zinc is to a certain limit directly proportional to the excess in the alloy, but in those of $SbZn_2$ the excess of antimony is far from obeying this rule; and were the excess in both cases a mechanical mixture the variation in both cases would undoubtedly follow the same law: again, the crystals of $SbZn_3$, take up an excess of zinc but do not take up an excess of antimony, while those of $SbZn_2$ crystallize with an excess of either, —facts which are as inconsistent with the idea of mechanical impurity as the last: finally the form of the curve of $SbZn_2$ of itself alone proves that the excess of antimony in the crystals is not in the condition of mechanical impurity; for in that case the variation of composition would not be influenced, as the curve shows that it is, by the chemical force.

A theory that the variation in composition resulted from the mixture of two or more isomorphous compounds would be even less tenable than the one just discussed. For in the first place it would be necessary to assume the existence of two other compounds of zinc and antimony isomorphous with $SbZn_2$ and of one other, if not more, isomorphous with $SbZn_3$. Not only would such an assumption be contrary to all the analogies of chemistry and therefore require strong evidence to sustain it; but in the second place it can almost be demonstrated that no such compounds exist. The crystals having the calculated composition of either $SbZn_3$ or $SbZn_2$ are marked as has been shown by striking peculiarities, and with one possible exception similar peculiarities were not observed throughout the whole series of crystals which have been examined. The crystals containing 50 per ct. of zinc and of the composition of $SbZn_4$ were found to have a slightly smaller sp. gr., than those just above or just below them, but the difference is so small that it may be accidental, and as the crystals exhibited none of the other peculiarities, which characterize crystals having the calculated composition of $SbZn_3$ or $SbZn_2$, I could not attach sufficient weight to the one circumstance to feel authorized in admitting a third compound of zinc and antimony. Admitting however the existence of $SbZn_4$ yet, as exactly the same angle has been observed in crystals containing 55 per cent. as on those containing 43 per cent of zinc, it would be necessary in order to explain the variation in composition by the principle of isomorphous mixtures, to assume the existence of still a third compound isomorphous with $SbZn_3$, and containing more zinc than $SbZn_4$, which would increase greatly the improbability of the theory in question. Again, the only probable compound of zinc and antimony containing less zinc than $SbZn_2$ would be $SbZn$; and it will be remembered that the crystals of $SbZn_2$ which contained the largest excess of antimony corresponded very nearly to this compound. In like manner the crystals of $SbZn_2$ which contained the largest excess of zinc corresponded very nearly to $SbZn_3$. If then the excess of antimony or zinc in the crystals of $SbZn_2$, arises from a mixture of isomorphous compounds, it must be that $SbZn_3$, $SbZn_2$ and $SbZn$ are isomorphous. That the first two are not isomorphous may be seen by turning back to the description of their crystalline form; and that there is no crystalline compound $SbZn$ is sufficiently proved by the fact that the crystals of $SbZn_2$, which correspond most closely to it, are so very imperfect that they would hardly be recognized as crystals did they not form the lower limit of a series. Several other facts pointing in the same direction might be added, but sufficient it is thought has been said to show that the variations of composition described in this paper can not be explained either by mechanical impurities in the crystals or by the mixture of isomorphous compounds.

In the absence of any known principle of chemical science by which the remarkable variations of composition, that have been demonstrated in this memoir, can be explained, the conclusion is almost forced upon us that zinc and antimony are capable of uniting and producing definite crystalline forms in other proportions than those of their chemical equivalents: in other words, that the law of definite proportions is not so absolute as has been hitherto supposed. The explanation then of the variation of composition which I would offer is: that it is due to an actual perturbation of the law of definite proportions produced by the influence of mass. I suppose for example that in the crystals of $SbZn_3$, containing 55 per cent. of zinc, the zinc and antimony are united in exactly the same way as in those containing 43 per cent., or in other words, just as if the equivalent of zinc were increased to 52·57, that of antimony remaining the same. In support of this position I would offer two considerations. The first is that if the variation is not caused by mechanical impurities or by the mixture of isomorphous compounds, we can conceive of no other explanation for the phenomenon than the one offered. This of course is merely negative evidence; for although science as yet presents us with no principle for explaining variations of composition other than those which have been discussed, and although we can conceive of none others, it does not follow that others may not exist or may not hereafter be discovered; but, nevertheless, this consideration is important inasmuch as it meets an obvious objection, which would be urged against any new doctrine, which conflicts with a generally received canon of chemical philosophy. The second consideration has the character of demonstration. It is that the curve of variation is evidently generated by a second force counteracting directly the chemical force. This second force, as has been shown, is exerted by the excess of one or the other element present in the menstruum, and it may therefore be appropriately termed the force of mass. While the chemical force tends to make the curve a straight line parallel to the axis of ordinates, the force of mass would reduce it to a straight line making an angle of 45° with the axis; under the influence of both these forces it follows the arc of a circle between the two. Now I urge that the character of this curve proves that the chemical force has been directly influenced by what we have called, the force of mass, in the same way that the irregularities of the orbits of the planets prove that the force of gravitation exerted by the sun has been disturbed in its action by the influence of the other members of the system. As the details in the form of the curve have been fully discussed in the previous part of the memoir, it does not seem to be necessary to dwell upon this argument, and I would therefore without further comment offer the curve as it has been laid down on the plate as the proof of the

validity of the explanation of the variation in composition here advanced.

It is worthy of remark that while the curve of variation may be said almost to demonstrate that the law of definite proportions may be disturbed in its action, it also most clearly sustains the integrity of the law itself; for, as may be seen on inspection, the chemical force is sufficiently strong to retain the curve of Sb Zn_2 parallel to the axis of ordinates through a variation in the menstruum of nearly five per cent., and it is only when the excess of antimony present in the alloy exceeds six per cent. that the force which it exerts becomes strong enough to disturb the action of the law. What the nature of the disturbing force is must be for the present a matter of theory. I am inclined to think that it is a phase of the chemical force itself, in the same way that the perturbations in the motions of the planets are a secondary result of the force of gravitation.

Accepting the view of the subject, which has been offered, it will be obvious that the very large extent of the variation in the compounds of zinc and antimony is due to the very weak affinity between these elements. Were the chemical force stronger in proportion to the disturbing force the variation would be lessened; were it weaker, the variation would be increased. This is illustrated in the difference between the curve of Sb Zn_3 and that of Sb Zn_2. It is evident from the action of chemical agents on the two compounds, that one equivalent of antimony and two of zinc are united by a stronger force than one equivalent of antimony and three of zinc, and we find that the crystals of Sb Zn_2 retain the calculated composition under a considerable variation in the composition of the menstruum, while the composition of those of Sb Zn_3 varies with the slightest increase of the amount of zinc in the alloy.

To what extent this perturbation of the law of definite proportions prevails among chemical compounds it must remain for future investigation to determine. There are however a number of facts which tend to prove that it is very general whenever chemical affinity is weak. Four of these I will cite as being remarkably analogous to the facts under discussion.

1. Rieffel, to whose investigation of the compounds of tin and copper we have already referred, says, after the paragraph quoted in the introduction to this memoir, "Les aiguilles de $CuSn_{24}$ sont plus grosses que celles de $CuSn_{48}$". "*On croit, sans oser l'affirmer, qu'elles sont, par compensation, en nombre moindre, et que des differences analogues ont lieu dans les autres* $CuSn_{\varphi}$ *à mesure que* φ *augmente jusqu' à* $\varphi = \infty$, *ou jusqu' à l'étain pur.*" It will be noticed that the difference between these needles is precisely the same as the difference between the crystals of Sb Zn_3 containing a small and a large amount of zinc, and I think

that no one after reading Rieffel's paper can doubt that the compounds of copper and tin vary in composition like those of zinc and antimony.

2. The mineral Discrasite, a compound of silver and antimony, crystallizes in trimetric prisms, of which I on $I = 119° 59'$.* The analyses given below are copied from Dana's System of Mineralogy, changing slightly the order.

$Sb Ag_3$ = Antimony 28·5 Silver, 71·5=100. $Sb Ag_4$ = Antimony 23, Silver 77=100.

1.	Andreasberg (foliated granular),	Antimony	24·25	Silver	75·25=99.5,	*Abich.*
2.	Wolfach (coarse granular),	"	24	"	76=100,	*Klaproth.*
3.	Andreasberg (foliated granular),	"	23	"	77=100,	"
4.	" " "	"	22	"	78=100,	*Vauquelin.*
5.	Wolfach (fine granular),	"	16	"	84=100,	*Klaproth.*

It needs no comment on these results to show that Discrasite is homœomorphous with $Sb Zn_3$, and varies like it in composition.

3. In a paper recently published,† W. Sartorius von Waltershausen gives descriptions and then analyses of a new mineral occurring with Dufrenoysite in the Binnen Valley, Switzerland, in Dolomite. As the analyses do not agree with each other and do not correspond to a simple formula, von Waltershausen regards the compound as consisting of two hypothetical isomorphous compounds $Pb S + As S_3$ and $2Pb S + As S_3$ and calculates the proportions in which these compounds are mixed in the specimens analyzed. He infers that they are isomorphous from their analogy in composition to Zinkenite and Heteromorphite, $Pb S + Sb S_3$ and $2Pb S + Sb S_3$ which he regards as isomorphous. Prof. J. D. Dana questions the isomorphism of the last and thinks that the hypothesis that the new compounds are isomorphous requires further evidence.‡

4. It is stated by Staedeler§ that crystals of the compound of grape sugar and common salt can be obtained containing for every equivalent of grape sugar one or two equivalents of chlorid of sodium and also of intermediate composition. He states moreover that "Calloud, who first observed that the grape sugar of honey combined with chlorid of sodium, found that the amount of the latter varied between 8·3 and 25 per cent." Staedeler refers the variation in composition to a mixture of the compound of one with the compound of two equivalents of chorid of sodium which he assumes to be isomorphous. He adds that it may be caused by "enclosed crystals of chlorid of sodium although the eye could not distinguish any heterogeneous constituents."

All the above compounds are examples of weak chemical affinity accompanied by large variations in composition without any

* Dana's System of Mineralogy, 4th ed., vol. ii, p. 35.
† Poggendorff's Annalen, vol. xciv, p. 115.
‡ American Journ. of Science, vol. xix, p. 355.
§ Chemical Gazette, vol. xiii, p. 44.

change in the general crystalline form. It is not meant to assert that the variations are identical in character with those of $Sb Zn_3$ and $Sb Zn_3$, but only that there is a strong probability that this is the case, which in the first two instances amounts almost to a certainty.

If variations in composition of such magnitude are possible when the force of chemical affinity is weak, it is highly probable that some variation may occur when the force is strong, and, whatever view may be taken of the cause of the variation it will now become a matter of importance to ascertain whether many discrepancies in analyses hitherto referred to imperfections in the process, may not be owing to the same cause which influences the composition of the crystals of zinc and antimony. For this purpose, it will be best to make several analyses of the same compound, prepared under circumstances differing as widely as possible, and then to apply to the results "Peirce's Criterion for the rejection of doubtful observations." Such investigations will be greatly simplified by tables prepared by Dr. B. A. Gould* for facilitating the application of this criterion to which I would refer all chemists who are inclined to take up this line of investigation.

I am well aware that in announcing the existence of perturbations of the law of definite proportions I am calling in question one of the most fundamental dogmas of chemical philosophy, and that the new doctrine will have to encounter prejudice on this very ground. This law is so intimately associated in many minds with the atomic theory, that, to such, absolute definiteness seems to be its essential characteristic; nevertheless, I can not but believe that, laying aside the prejudices which the theory begets, it will be seen by all, that the analogies of nature support the doctrine of variation as maintained in this memoir. The phenomena of some of the phenomenal† laws of nature have that definiteness of character which is claimed for those of the chemical law. The planetary orbits are not perfect ellipses. The ratios of the harmony scale are but approximately realized. The arrangement of leaves on the stem is not perfectly regular. Isomorphism is seldom absolute. In all we observe only a tendency towards a maximum effect which is the perfect expression of the law, but which is rarely fully reached. The limits of variation are broader in some instances than in others, but we find no case in which there is absolutely none. This same character which pervades the other phenomenal laws of nature, I claim for

* Astronomical Journal, vol. iv, p. 81.

† I have used the term phenomenal laws to designate a class of laws of nature which are empirical in their character inasmuch as they are obviously not ultimate, although their deviation has not been discovered, but which are more universal than those to which the term empirical is commonly applied.

the great law of chemistry. The definite proportion I regard as a maximum toward which the chemical force strives, a maximum, from which the deviations in most cases are small, although in others it may be very large; and I maintain that this view of the subject, which the memoir has aimed to establish, is supported by the analogies of nature.

When the dynamical law has been discovered, of which the phenomenal law was merely the outward manifestation, as Kepler's laws were merely the phenomena of the law of universal gravitation, the very variations have been seen to be necessary consequences of the law itself, and if even the dynamical law, which governs chemical phenomena, shall be discovered, it is most probable that the variations from the law of definite proportions will become as much a matter of calculation as the perturbations of Astronomy. In both cases the perturbation is apparently due to the influence of an extraneous mass of matter.

The argument from analogy becomes stronger, when we consider the equivalent numbers. I have shown in a former memoir* that these numbers may be connected by a very simple numerical law, but here, as in other cases, we find merely a tendency towards the law, not an absolute agreement with it, the differences between the theoretical and the experimental equivalents being in many cases too great to be covered by errors of observation. The present memoir may throw light upon these discrepancies; for, to say the least, it is possible that the differences may originate in variations of the equivalent itself, and that the theoretical equivalent may be the maximum towards which the chemical force tends. On comparing carefully the different determinations of the chemical equivalents, many facts may be noticed supporting this view; those equivalents for example, which coincide with or very nearly approach whole numbers such as those of oxygen, carbon and sulphur, will be found as a general rule to have been determined by the analysis or synthesis of compounds whose elements are united by a strong force; also when the equivalents have been determined by essentially different processes it will be noticed that they seldom perfectly agree; so that whatever view may be taken of the subject it will now become a matter of the highest importance to ascertain how far, if at all, the determinations of the chemical equivalents have been influenced by similar causes to those which have produced the variations described in this memoir. This influence can only be detected by multiplying the determinations, by as many different processes as possible, and submitting the results to a rigorous mathematical scrutiny.

If the doctrine of this memoir is correct and the chemical equivalents are really liable to variation it will have an important

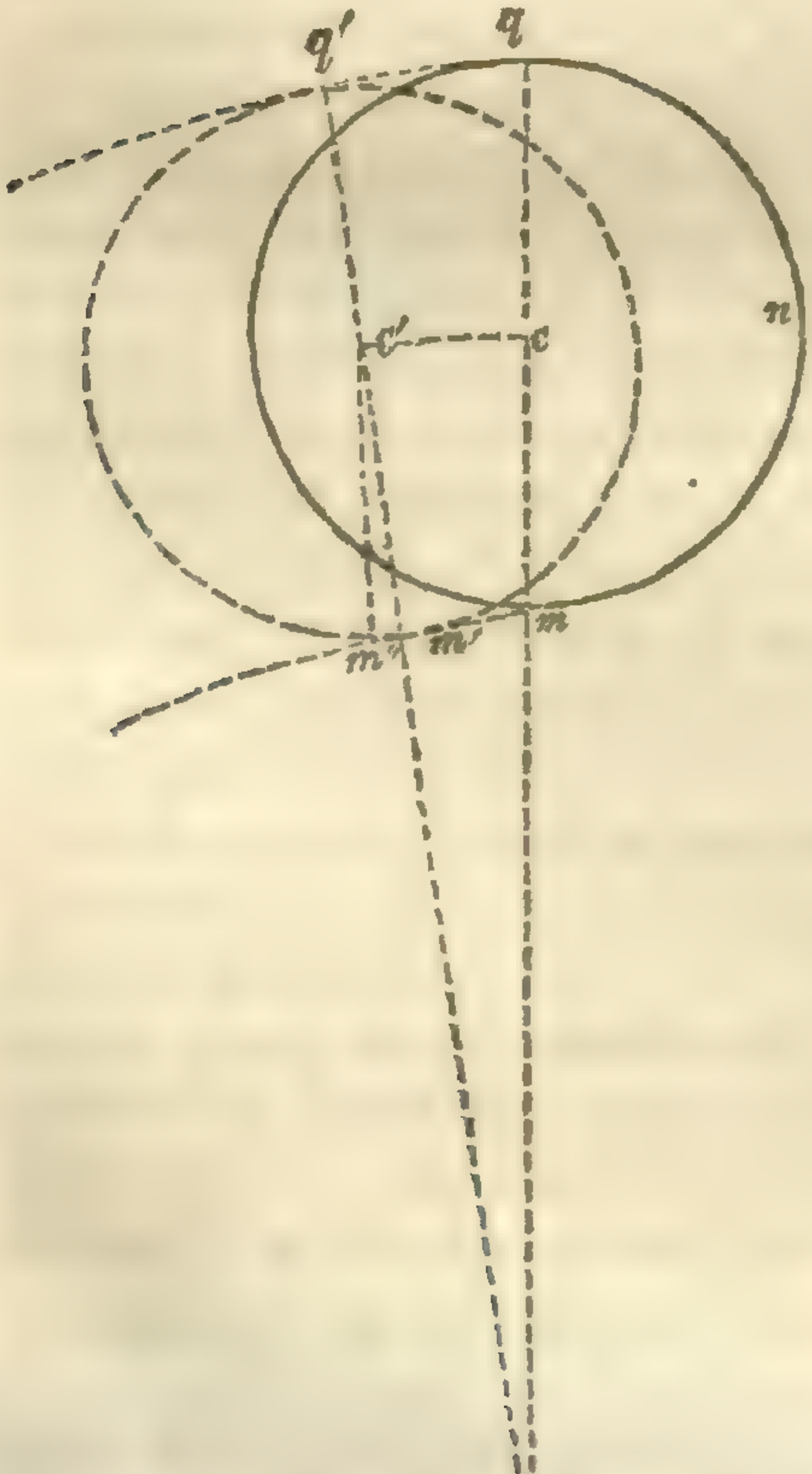
* This Journal, vol. xviii, p. 229,

influence on chemical philosophy. The atomic theory as at present interpreted by chemists is irreconcilable with it, and our present ideas in regard to isomorphism must be materially changed. But it must be remembered that the conclusions of the memoir are drawn from the examination of only two compounds, and therefore that it would be premature to dwell on these obvious consequences of the principle until it has been substantiated by further investigations. In conclusion, I would express my obligation to the gentlemen who have assisted me in the labor of the investigation which on account of the large number of analyses has been very great and could not have been concluded so soon had it not been for their great industry and zeal.

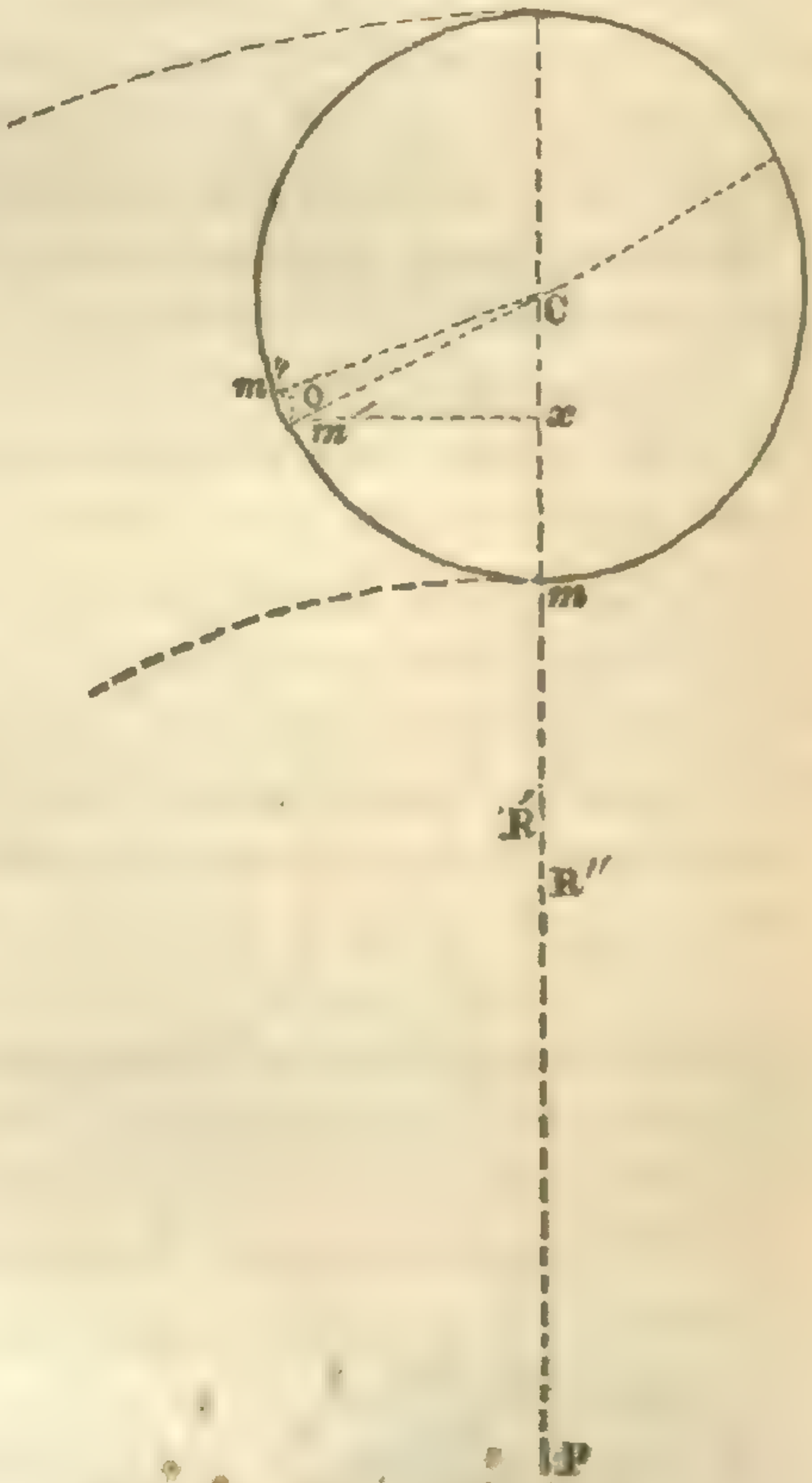
ART. XXIII.—*Demonstration of the Apparent Motion of the plane of Oscillation of the Pendulum, due to the Earth's Rotation;* by J. G. BARNARD, A.M., Brevet Maj. Engineers U.S.A.

LET P be the pole of the earth, and C be the central point of a dial represented by the circle *mnq** over which the pendulum swings; the whole projected upon the plane of the equator.

1.



2.



* The dial circle would of course, be projected, on the plane of the equator in an ellipse; but for convenience is represented as a circle.

In the reasoning which follows the radius C*m* representing the semi-oscillation of the pendulum is supposed, as indeed it always is, exceedingly minute compared with the earth's radius; it is also very minute compared with CC′ which measures the rotary movement of C during a semi-oscillation.

C being the centre of motion, the bisecting point of each arc of oscillation, its *rotary* velocity about the earth's axis will evidently be that of the pendulum itself.

Even if we suppose the pendulum as commencing its series of oscillations at either of the points *q* or *m* and thence having *their* rotary velocities, the very first vibration will check or accelerate it to that of C.

While the pendulum is moving from, C to *m*, suppose the centre of motion C to move to C′ by the earth's rotation; the point *m* of the dial; will only have the velocity due to the radius of the earth's small circle P*m*. Its motion will be *mm′* while that of C is CC′ due to the radius PC.

But the pendulum having the rotary velocity of the point C will advance, in rotation a distance, from its original plane *mq*, equal to CC′. Instead therefore of striking the dial circle at *m′* it will strike it in advance of it a distance *m′m″* equal to CC′−*mm′*.

That is to say, *the distance the pendulum advances each vibration is equal to the difference of the arcs of the circle described about the earth's axis by the points* C *and m, during the vibration.*

The foregoing proposition has been derived from the consideration of the pendulum vibrating in the plane of the meridian.

But it can be established in a more general way by considering a semi-vibration in any other plane C*m′*. (Fig 2.)

In this case the pendulum will gain in the *direction of rotation* upon the point *m′* (where it would strike the dial circle, if the earth was motionless) a distance *om″* in the *direction of rotation* due to the difference of rotary velocities of the points C and *m′* or *x*. Such an increment in the ordinate from *x* will cause an increment *mm′* of arc the same as just found for the vibration in the plane of the meridian.

For, call V the rotary velocity of C, the time of a semi-oscillation, *t*: the radius PC=R′ and that P*m*=R″. (Fig. 1.)

The rotary velocity of the point *m* will be $\frac{R''}{R'}V$, and the *difference of velocities* of C and *m*, $V\left(\frac{R'-R''}{R'}\right)$. The distance *m′m″* gained by the pendulum will therefore be (in case of vibration in the plane of the meridian) $=Vt\left(\frac{R'-R''}{R'}\right)$.

For the case of oblique vibration (Fig. 2), call $mx = x$ (x being an abscissa of the dial circle), and the ordinate $xm' = y$.

The point of the dial m' will have a velocity $= \frac{R'' + x \sin \Phi}{R'} V$ (Φ being the latitude) and the difference of rotary velocities between it and C will be $V\left(1 - \frac{R'' + x \sin \Phi}{R'}\right)$ and the distance om'' gained, in the direction of rotation, by the pendulum, will be, $Vt\left(1 - \frac{R'' + x \sin \Phi}{R'}\right)$.

But the increment of abscissa om' corresponding to the increment of ordinate om'' will be determined by the equation of the circle, these increments (being very small) being as the differentials of y and x.

Call $r \left(= \frac{R' - R''}{\sin \Phi}\right)$ the radius of the dial circle, then

$om' : om'' :: dx : dy :: \sqrt{2rx - x^2} : r - x$; hence $om' = om'' \frac{\sqrt{2rx - x^2}}{r - x}$

$= Vt\left(1 - \frac{R'' + x \sin \Phi}{R'}\right) \frac{\sqrt{2rx - x^2}}{r - x} = \left(\text{since } r = \frac{R' - R''}{\sin \Phi} \text{ and hence } 1 - \frac{R'' + x \sin \Phi}{R'} = \frac{r - x}{R'} \sin \Phi\right) Vt \frac{\sqrt{2rx - x^2}}{R'} \sin \Phi.$

Hence

$$\overline{m'm''}^2 = \overline{om''}^2 + \overline{om'}^2 = V^2 t^2 \left[\frac{(R' - R'' - x \sin \Phi)^2}{R'^2} + \frac{2\left(\frac{R' - R''}{\sin \Phi}\right) x - x^2}{R'^2} \sin^2 \Phi\right]$$

which, being developed, reduces to

$$V^2 t^2 \frac{(R' - R'')^2}{R'^2}, \text{ hence } \overline{m'm''} = Vt \frac{R' - R''}{R'},$$

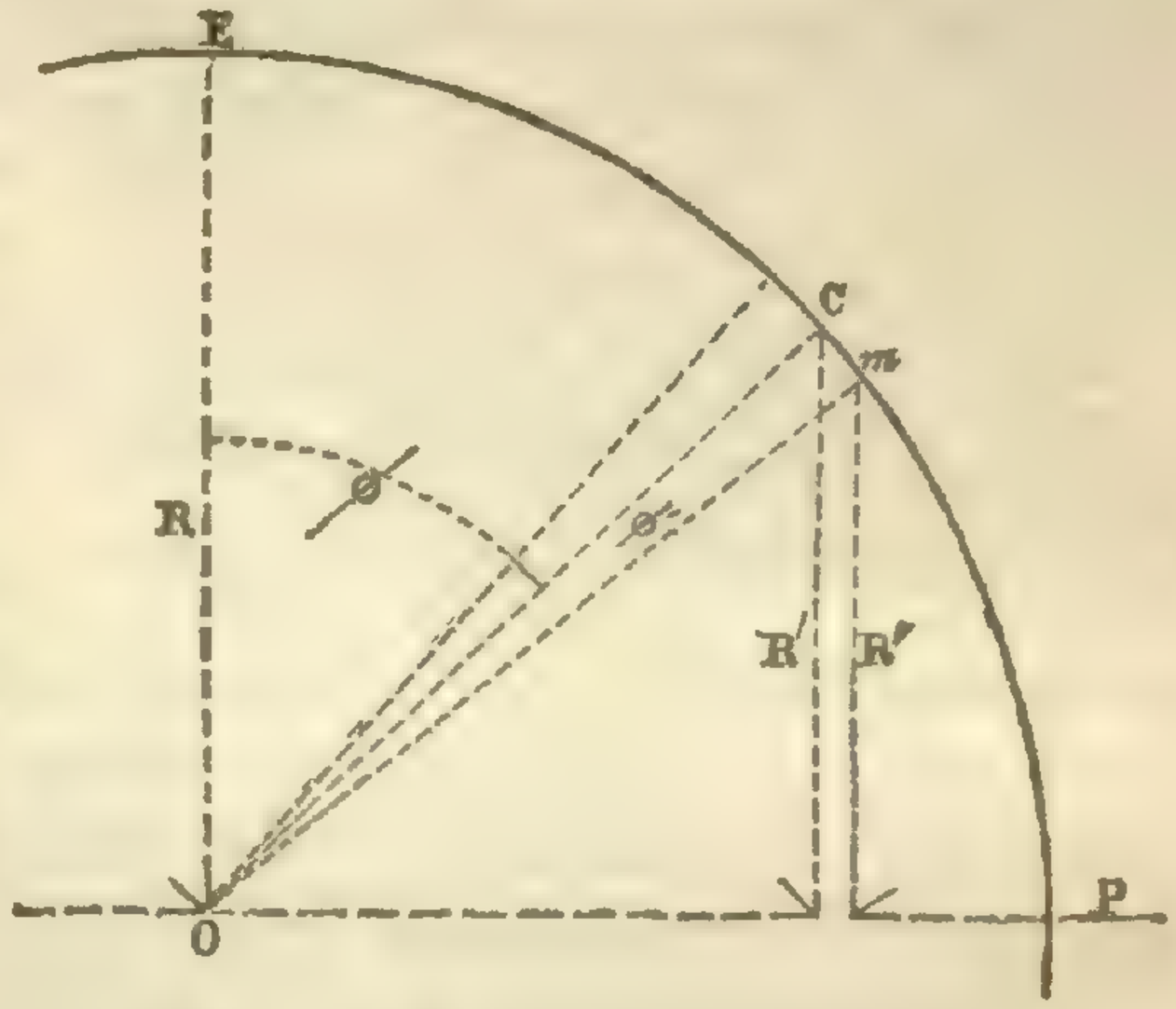
an expression the same as obtained for the case of vibration in the plane of the meridian, and constant for all positions of the plane of vibration.

It is then proved that, at each vibration, the plane of vibration will shift on the dial through an angle whose arc, on the dial circumference, will be equal to the difference between the arcs of rotation described during the same time by the centre and extremity of the meridional diameter of the dial.

After a complete revolution of the earth, the length of the arc of the dial passed over, or which measures the angular change of plane of vibration will be, evidently, equal to the difference be-

tween the total circumferences of the two small earth's circles, just mentioned.

3.



Let P*mc* be a meridional section, EO the equator, Φ latitude of C, and Φ' half the angle subtended by the chord of vibration C*m*, and R the earth's radius.

R′ and R″ are the radii of the circles of rotation of the points C and *m*; then $R' = R\cos\Phi$, $R'' = R\cos(\Phi+\Phi')$ and

$$\text{circumference } R' = 2\pi R\cos\Phi$$

$$\text{“} \qquad R'' = 2\pi R\cos(\Phi+\Phi').$$

We have proved that the length of the arc of the dial moved over by the pendulum in 24 hours is equal to the difference between these circumferences

$$\text{circ } R' - \text{circ } R'' = 2\pi R\,(\cos\Phi - \cos(\Phi+\Phi')) =$$
$$2\pi R\,(\cos\Phi - \cos\Phi\cos\Phi' + \sin\Phi\sin\Phi')$$

where Φ' (as is actually the case) is extremely minute, $\cos'\Phi$ is sensibly $=1$ and the above expression becomes

$$\text{circ } R' - \text{circ } R'' = 2\pi R\sin\Phi\sin\Phi'.$$

But $R\sin\Phi' = Cm$ and $2\pi R\sin\Phi'$ is therefore equal to the circumference of the dial circle.

$2\pi R\sin\Phi\sin\Phi'$ is therefore equal to the dial circumference, or 360° multiplied by the sine of the latitude.

In other words the plane of oscillation will move in 24 hours through an angle equal to 360° × sine of the latitude:— a result arrived at by other methods and proved by observation.

The foregoing demonstration is rigid; it is based upon the simple fact that the pendulum has (with its point and axis of suspension) a rotary motion of its own, around the axis of the earth; and that while this rotary movement is preserved unchanged, the oscillation of the pendulum is governed by the same laws in reference to its *axis of suspension* as if both were motionless in space; a supposition on which all the investigations of the phenomena of gravitation, as applied to the earth and heavenly bodies, are based.

It *assumes nothing* unless it be, that the mere *angular motion* which the direction of the axis of suspension and of gravity together undergo, will not permanently alter the direction of the chord of the arc of oscillation.*

* This assumption is capable of proof but it is not worth while to complicate the subject by introducing it here.

ART. XXIV.—*Reëxamination of American Minerals:* PART V. —*The Minerals of the Wheatley Mine in Pennsylvania.*—*Anglesite; Cerusite; Wulfenite; Vanadate of Lead; Pyromorphite; Mimetene; Galena; Copper; Copper Pyrites; Malachite; Azurite; Blende; Calamine; Hematite; Fluor Spar; Calc Spar; Sulphur*, &c.; by J. LAWRENCE SMITH, M.D., Prof. Chem. Med. Depart. University of Louisville.*

(Communicated to the Am. Scientific Association, August, 1855.)

BEFORE describing the minerals of this mine, it is well to say a word with reference to its location, and also to quote some remarks on the geology of the surrounding country by Prof. H. D. Rogers. Although this is departing from the plan usually adopted in this series of papers, still the occurrence of all the minerals, here described, at one locality, cannot but render the geology of the place interesting to mineralogists.

This mine is situated in Chester Co., near Phœnixville, Pennsylvania, and is one of several interesting developments of a thorough and very able exploration of this region by Mr. Charles M. Wheatley. At the request of Mr. Wheatley, Prof. Rogers made a geological examination of the metalliferous veins of this district, and the following remarks are taken from his report.

"These veins belong to a group of lead and copper-bearing lodes of a very interesting character, which form a metalliferous zone, that ranges in a general east and west direction across the Schuylkill River, near the lower stretches of the Perkiomen and Pickering Creeks in Montgomery and Chester Counties, and bids fair to constitute at no distant day a quite productive mineral region.

"The individual veins of this rather numerous group, are remarkable for their general mutual parallelism, their average course being about N. 31°—35° E. by compass, and not at all coincident with that of the belt of country which embraces them. They are true lodes or mineral injections, filling so many dislocations or fissures, transverse to the general direction of the strata which they intersect. The metalliferous belt ranges not far from the boundary which divides the gneissic or metamorphic rocks of Chester County from the middle secondary red shale and sandstone strata."

"This vein varies in thickness from a few inches to about two and a half feet, nnd we may state its average width at not less than eighteen inches. It is bounded by regular and well-defined, nearly parallel walls, the prevailing material of which is a coarse, soft granite, composed chiefly of white feldspar and quartz.

"It would seem to be a pretty general fact that such of these veins as are confined entirely or chiefly to the gneiss, bear *lead* as their principal metal; whereas, those which are included solely within the

* I am indebted to Prof. J. D. Dana for the figures and mathematical descriptions of the crystals given beyond.—J. L S.

red shale are characterised by containing the ores of copper. But the zinc ores, viz., zinc blende and calamine, prevail in greater or less proportions in both sets of veins, existing, perhaps, in a rather larger relative amount in the copper-bearing lodes of the red shale."

"The *gneissic strata* of the tract embracing this group of lead-bearing veins, seem to differ in no essential features from the rest of the formation ranging eastward and westward through this belt of country. Here, as elsewhere, they consist chiefly of soft thinly bedded micaceous gneiss, a more dense and ferruginous hornblendic gneiss, and thirdly a thicker bedded granitic gneiss, composed not unfrequently of little else than the two minerals, quartz and feldspar.

Penetrating this quite diversified formation are innumerable injections of various kinds of granite, greenstone trap, and other genuine igneous rocks. The granites, as throughout this region generally, consist for the most part, of a coarse binary mixture of quartz and opaque white feldspar, tending easily to decomposition. This rock abounds in the form of dykes and veins, sometimes cutting the strata of gneiss nearly vertically, but often partially conforming with its planes of bedding for a limited space, and then branching through, or expiring in it in transverse or tortuous branches. A not uncommon variety of granitic dyke is a simple syenite composed of quartz, greenish semi-translucent feldspar, and a smaller proportion of dark green hornblende. A soft, white, and partially decomposed granite is a very frequent associate of the stronger lead-bearing veins, particularly in their more productive portions; but this material belongs, in all probability, not to the ancient granitic injections of the gneiss, but to those much later metalliferons intrusions which filled long parallel rents in that formation with the lead ores and their associated minerals."

"The gneissic strata and their granitic injections, throughout this district display a softened, partially decomposed condition, extending in many places to a depth of twenty fathoms."

"Of the dozen or more lead and copper lodes of greater or less size brought to light in this quite limited region of five or six miles length and two or three miles breadth, the greater number are remarkably similar in their course, ranging N. 32°—35° E., and S. 32°—35° W.; and what is equally worthy of note, they dip, with scarcely an exception, towards the same quarter, or south-eastwardly, though in some instances so steeply as to approach the perpendicular."

"There is no marked difference in the general character of the veinstones of the several mineral lodes, nor any features to distinguish as a class those of the red shale from those of the gneiss."

The minerals found in these veins are quite numerous, and among them, there are specimens of species hardly equalled by those coming from any other locality. Prof. Silliman, in his report on the minerals of this mine exhibited at the Crystal Palace, says, that the specimens of sulphate and molybdate of lead are the most magnificent metallic salts ever obtained in lead mining, and unequalled by any thing to be seen in the cabinets of Europe.

48. *Anglesite.*

This mineral is found abundantly and in beautiful crystals at this locality. The magnificence of many of the specimens can only be realized by seeing those in Mr. Wheatley's cabinet. The crystals are remarkable for their size and transparency;—in some instances, they weigh nearly half a pound being as transparent as rock crystal in nearly every part. Crystals with terminations at both ends have been obtained five and a half inches in length by one and a half in thickness; perfectly limped crystals an inch in length are quite common.

The following are some of the forms:

1.—0, ∞, 1-$\breve{\infty}$.

2.—0, $\frac{1}{2}$-$\bar{\infty}$, ∞-$\bar{\infty}$, 1, α, 1-$\breve{2}$, 1-$\breve{\infty}$, ∞-$\breve{\infty}$.

3.—0, $\frac{1}{4}$-$\bar{\infty}$, $\frac{1}{2}$-$\bar{\infty}$, ∞, 1-$\breve{2}$, 2-$\breve{4}$

4.—0, $\frac{1}{2}$-$\bar{\infty}$, ∞-$\bar{\infty}$, $\frac{3}{4}$-$\frac{\breve{3}}{2}$, 1, ∞, 1-$\breve{2}$, 2-$\breve{4}$, 1-$\breve{\infty}$.

Sometimes the crystals of this mineral are full of cavities, and of a milk-white color; but these do not differ in composition from the colorless and transparent forms. It also occurs in circular crystals.

It is sometimes colored. There is a black variety produced by the more or less perfect admixture of the sulphurets of lead and copper (containing traces of silver) in the mass of the crystals, whose form is not altered. There are crystals of a delicate green color arising from carbonate of copper, and others of a yellow color due to oxyd of iron.

The transparent and colorless variety is remarkably pure. Its sp. grav. is 6·35. On analysis it afforded,

	1.	2.
Sulphuric acid,	26·78	26·61
Oxyd of lead,	73·31	73·22
Silica,	·20	
	100·29	99·83

according very precisely with the formula $\dot{P}b\breve{S}$.

I would call attention to the method of analyzing this sulphate, as described in another paper, for it was analyzed in the moist way by dissolving it first in citrate of ammonia.

The anglesite of this mine is found variously associated. It is common to find it in geodic cavities in galena, the cavities being lined with hematite varying in thickness from $\frac{1}{2\,0}$ to $\frac{1}{2}$ an inch or more, and often this hematite contains anglesite intimately mixed in the mass. It may occur in crystals occupying a portion of the geode, or it may fill its entire capacity, assuming the form of the cavity. It is also found compacted in the galena without the appearance of any cavity or the presence of any other mineral; acicular crystals occur diffused through the galena. Observed also on copper pyrites, with a thin layer of hematite inter-

vening between the crystal and the pyrites—on crystals of zinc-blende in quartz—on quartz associated with pyromorphite—on galena with crystals of sulphur—on calc spar without any associate. One very interesting specimen consists of a flattened crystal an inch square, having a delicate crystal of calc spar over an inch and a half in length perforating the centre and around which the sulphate appears to have formed. It is also found on fluor spar without associate.

Some of the most beautiful specimens are where large crystals of anglesite are covered with crystals of carbonate of lead, these latter frequently penetrating the anglesite.

49. *Cerusite.*

The crystals of this mineral, though not as large as those of anglesite, are yet exceedingly beautiful, both in size as well as transparency; the twin crystals are often two inches broad, transparent and presenting the appearance of the spread wing of a butterfly, some of the single crystals are an inch in length and half an inch thick.

A transparent crystal weighing five grammes, gave a sp. grav. of 6·60 and on analysis furnished

Carbonic acid,	16·38	} = Pb C
Oxyd of lead,	83·76	}
	100·14	

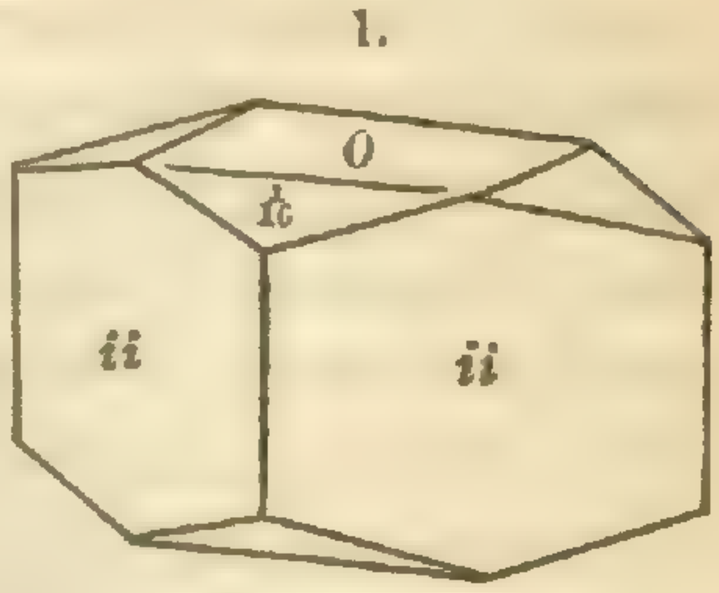
It occurs in hematite coating galena, in a manner similar to the anglesite, and associated with it: also in connection with pyromorphite, which often colors the entire body of the crystals of cerusite. It is found on galena without the association of any other mineral—on green and blue carbonate of copper—on pyromorphite which often covers the entire surface of the cerusite crystals, imparting to them an opaque yellowish green color—on oxyd of maganese in snow-white crystals, without any other associate—on hematite in a similar manner; mamillary masses of the hematite sometimes pass through the crystals Some few specimens have been found consisting of crystals of galena, with a number of very fine hemitrope crystals of cerusite on the surface. The cerusite is occasionally covered with an exceedingly thin coat of oxyd of iron giving the crystals a dark red appearance, and some of them again with a very thin layer of pyromorphite, as delicate as if it had been put on with a brush.

The cerusite is sometimes colored, black, green and yellow, in a manner similar to that mentioned under anglesite.

50. *Wulfenite.*

This mineral is found in small crystals of every shade of color from a light yellow to a bright red; it has been found in some

abundance, forming, from the manner of its occurrence, very beautiful specimens. The crystals present a variety of modified forms, tabular and octahedral, one of which is here figured.



Other forms are 0, 1.

0, $\frac{1}{3}$, 1.

0, $\frac{1}{16}$, ∞-∞. (Fig. 1.)

Specific gravity of a dark yellow variety, 6·95.

The composition of both the yellow and red varieties was examined; the difference of color is due to the presence of *vanadic acid* in the red varieties, and the intensity of color is proportional to the amount of vanadic acid, which in no instance is much more than one per cent.

The analyses afforded

	Yellow variety.	Red variety.
Molybdic acid,	38·68	37·47
Vanadic acid,	——	1·28
Oxyd of lead,	60·48	60·30
	99·16	99·05

The second corresponds very nearly to 97 p. ct. of molybdate and 3 p. ct. of vanadate of lead. As the last substance varies in quantity, it cannot be regarded as giving a distinct specific character to the mineral. This mineral has been described as a chromo-molybdate of lead, but by the most careful examination only a trace of chromium can be detected; in fact, the quantity is so minute, as to require further examination in larger quantities to place the matter beyond a doubt.

Wulfenite occurs alone on crystallized and cellular quartz, or associated with pyromorphite, whose beautiful green color is often very much enhanced by the contrast of the yellow and red crystals on its surface.

Sometimes the wulfenite forms the mass, and crystals of pyromorphite are sparsely disseminated over the surface. It is also found in decomposed granite—on carbonate of lead and oxyd of manganese—also associated with vanadate of lead.

51. *Vanadate of Lead* (*Descloizite?*)

This species has never before been remarked among American minerals, although the chloro-vanadate (vanadinite) was first discovered in Mexico. This adds another to the list of curious minerals from the Wheatley mine. It was noticed about a year ago in the form of a dark colored crystalline crust, covering the surface of some specimens of quartz and ferruginous clay associated with other minerals. Observed with a magnifying glass, it is seen to consist principally of minute lenticular crystals, grouped together in small botryoidal masses; the crystalline structure is perfect. Thus seen, the color of the mass is of a dark purple,

almost black. When seen by transmitted light, the color is dark hyacinth-red and translucent. The streak is dark yellow. From the difficulty of obtaining any quantity of sufficient purity, nothing accurate can be stated with reference to its specific gravity and hardness; and for the purpose of analysis, I was obliged to use material, which although containing pure crystals of the vanadate, was yet mixed with crystals of molybdate of lead and other impurities.

The chemical analysis is an imperfect one, yet the best that can be made from the mineral as it has been found. It is as follows:

Vanadic acid,	11·70
Molybdic acid,	20·14
Oxyd of lead,	55·01
Oxyds of iron and manganese, Alumina,	5·90
Oxyd of copper,	1·13
Sand,	2·21
Water,	2·94
	99·03

If we subtract the amount of oxyd of lead requisite to form Wulfenite with the molybdic acid present, we have left 22·82 per cent. which is combined with 11·7 of vanadic acid, making a compound corresponding to, Vanadic acid 66·1, oxyd of lead 33·9=100.

This result is not considered precise; it corresponds however, more nearly with the composition of Descloizite as given by Damour ($\dot{P}b^2\ \ddot{V}=\ddot{V}$ 29·3, $\dot{P}b$...70·7), than with Dechenite by Bergmann ($\dot{P}b\ \ddot{V}=\ddot{V}$ 45·34 $\dot{P}b$ 54·66).

The composition of Descloizite cannot be considered as having been fairly made out, for Damour's results are deduced, as mine have been, from a very impure material, and may on future examination prove to be $\dot{P}b^3\ \ddot{V}^2$; corresponding in composition to the chromate of lead called Melanochroite. This mineral has as yet been found only in small quantity at this mine, associated with oxyd of manganese and wulfenite, the crystals of this latter substance being more or less covered with minute crystals of the vanadate.

52. *Pyromorphite.*

There are several shades of color belonging to this mineral, a green so dark as to be almost black, olive-green, pea-green, leek-green, greenish yellow and all intermediate shades. It is a very abundant ore at the Wheatley Mine, and large quantities of it are smelted. Specimens of great beauty are found occurring in botryoidal masses with columnar structure, in perfect hexagonal prisms with the summits more or less modified. Crystals are found ½ inch in diameter. Some of the crystals are hollow with only an hexagonal shell; sometimes the crystals are agglomerated in a plumose form.

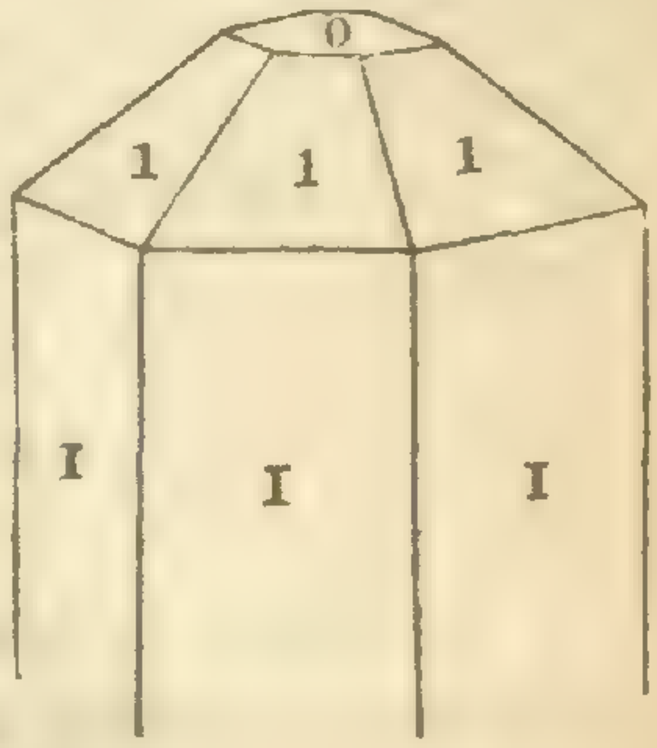
A dark green variety gave a sp. grav. of 6·94. No analysis was made of this mineral, as it will be embraced in an examination of the American pyromorphites to be published at some future time.

It is found in decomposed granite, on quartz crystals, occasionally covering their entire surface; in cellular quartz with molybdate of lead; in large masses of grouped crystals with small crystals of yellow and red molybdate inserted on crystals of sulphate and carbonate of lead, and forming a coating to large surfaces of galena.

53. *Mimetene.*

The specimens of this mineral that have been found, although few in number, are remarkable for their beauty of crystallization. Some of the crystals are nearly colorless and perfectly transparent, others of a lemon-yellow, either pure or tinged with green. The form is that of a perfect hexagonal prism, the edges of the summit most commonly truncated, often to such an extent as to terminate the crystal with a hexagonal pyramid, (fig. 2). The crystals are sometimes as small as a hair, and a quarter of an inch or more in length, and again they are so broad and short as to form hexagonal plates half an inch across.

2.



A specimen of the lemon-yellow variety was examined; it gave a sp. grav. of 7·32, and was found to contain,

Arsenic acid,	23·17
Chlorine,	2·39
Oxyd of lead,	67·05
Lead,	6·99
Phosphoric acid,	·14
	99·74

corresponding to, Arsenate of lead 80·21, chlorid of lead 9·38=

$$\dot{P}b^3\,\ddot{A}s + \tfrac{1}{3}Pb\,Cl$$

This specimen of mimetene is seen to be almost free from phosphoric acid, containing only about $\frac{1}{10}$ of a per cent., in this respect resembling that from Zacatecas as analyzed by Bergmann.

This mineral is found in granite or quartz. It is also associated with pyromorphite and sometimes the two run together, so as to present no distinct line of demarcation between them; some of the specimens consist of the two minerals, the pyromorphite forming one entire surface, and mimetene the opposite surface, and between, various shades of the mixture. It has been found with galena and carbonate of lead.

54. *Galena.*

The compact, fibrous and crystallized varieties of galena occur at this mine. Fine crystals are found, either a perfect cube, cube

with modified edges and angles, octahedron and rhombic dodecahedron often very much flattened out and occasionally rounded to an almost globular form; these rounded crystals are usually covered with pyromorphite. The galena is sometimes cellular, arising from partial decomposition, the exterior portion presenting a black drusy appearance, the interior of a bright steel color; this variety is particularly rich in silver and also contains crystals of sulphur.

The galena is argentiferous, giving an average yield of thirty ounces to the ton. It is found associated with quartz, calcite and fluor spar, frequently inserted in the crystals of these substances; it is also a common associate of all the minerals of this locality. Some of the cubical crystals have their surfaces partly decomposed and covered with a layer of crystals of carbonate. Specimens are found of very large cubical and octahedral crystals forming slabs several square feet in surface completely covered with a layer of leek-green phosphate. The cavities of the galena frequently contain sulphur.

55. *Copper.*

Native copper is found only in delicate films on hematite, or quartz crystal, and forms an interposing layer between the hematite and copper pyrites.

56. *Copper Pyrites.*

Copper pyrites is found in some cases in sufficient quantity to be worked as an ore; some of the masses are of considerable size weighing three or four hundred pounds. Fine crystals are obtained, both tetrahedral and octahedral. It affords on analysis

Sulphur,	36·10
Copper,	32·85
Iron,	29·93
Lead,	·35
	99·23

It occurs alone and associated with the other sulphurets. It is found in various parts of the vein, there being no special point of deposit.

57. *Malachite.*

Malachite occurs in small reniform masses, consisting of fibrous crystals, and of a bright green color; also in silky tufts of a very light green color, which are associated with azurite and carbonate of lead. Its sp. grav. is 4·06. An analysis gave:

Carbonic acid,	19·09
Oxyd of copper,	71·46
Water,	9·02
Oxyd of iron,	·12
	99·69

affording the formula $\dot{C}u\,\ddot{C}+\dot{C}u\,\dot{H}$

It is associated with the various ores of copper and lead of the Wheatley mine, and sometimes so thoroughly diffused through the sulphate and carbonate of lead as to give them a uniform green tint. It is not found in any quantity.

58. *Azurite.*

This mineral, although rare, is found in beautiful crystals some measuring from $\frac{1}{4}$ to $\frac{1}{2}$ inch across, of a deep blue color, and highly polished faces. Its sp. grav. is 3·88. An analysis gave,

Carbonic acid,	24·98
Oxyd of copper,	69·41
Water,	5·84
	100·23

giving the formula $2\dot{C}u\,\ddot{C}+\dot{C}u\,\dot{H}$.

This species occurs in similar associations with the malachite. It is however rarer.

59. *Zinc Blende.*

Blende is found in considerable quantity both massive and crystallized. Some of the crystals are exceedingly beautiful, and of large size, being three or four inches in diameter and with very brilliant surfaces. The colors are dark hair-brown and black, the brown being transparent. The specimens from this locality are hardly surpassed by those from any other mine. A specimen that was analyzed gave the following results:

Sulphur,	33·82
Zinc,	64·39
Cadmium,	·98
Copper,	·32
Lead,	·78
	100·29

It is proposed to examine yet other specimens, to see if there may not be larger amounts of cadmium contained in some of them.

This mineral occurs in fluor spar, calc spar and quartz, more or less mixed with the other sulphurets. In some instances it is very peculiarly interlaced in the rocks; thus we have specimens consisting as it were of four layers, namely, granite, then compact crystallized quartz $\frac{3}{4}$ths of an inch thick, then the blende an inch thick, on that a layer of crystals of calc spar, and on this last fluor spar.

60. *Calamine.*

Calamine is found in delicate crystals of a silky lustre, forming in some instances snow-white tufts on fluor spar, blende and carbonate of lime. It is also found on cellular quartz. Some of the specimens are quite handsome, having a blue and yellow color from the presence of carbonate of copper and oxyd of iron. No analysis was made of any of the specimens.

61. *Brown Hematite.*

This ore occurs in concretionary masses of a dark liver color, and compact structure, associated with nearly all the minerals of this mine—it very commonly forms a lining to cavities in galena, in which are found crystals of anglesite and cerusite; sometimes it lines cavities in the rock that are completely filled with cubical galena. Acicular concretions of the hematite are found traversing crystals of anglesite and cerusite. A specimen of the purest hematite gave for its composition

Peroxyd of iron,	80·32
Oxyd of copper,	·94
Oxyd of lead,	1·51
Water,	14·02
Silica,	3·42
	100·21

62. *Fluor Spar.*

The remarkable feature of the fluor spar of this mine is the absence of color; all the specimens yet found being colorless and transparent. The crystals are very perfect and beautiful, yet small; it is sometimes in globular concretions, of crystalline structure radiating from the centre. The cube, which is the more common crystalline form, is sometimes very much modified by the truncation of the edges and angles. A specimen that was examined gave a sp. grav. of 3·15, and the following composition:

Fluorine,	48·29
Calcium,	50·81
Phosphate of lime,	*a trace*
	99·10

It is associated with calc spar, and in some instances in a remarkable manner, mentioned under the head of calc spar. Galena and blende are interspersed through it. Its occurrence in the mine was first noticed at the depth of three hundred feet, and since then it has been found abundantly.

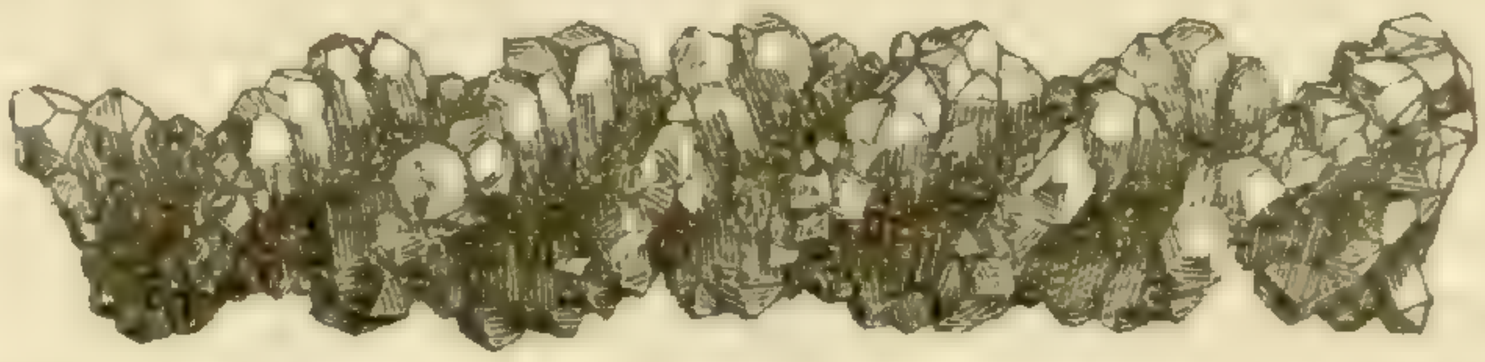
63. *Calc Spar.*

There are a variety of interesting forms and associations of this mineral. The two most common are the dog-tooth spar and the hexagonal prism with a three-sided summit, and occasionally the hexagonal prism with flattened summits like arragonite. Sometimes slabs of this mineral are found, with a surface of eight or ten square feet completely covered with prismatic crystals an inch or two in length, and from ½ an inch to an inch in thickness; they are mostly vertical, but occasionally horizontal with double terminations. These crystals are sometimes of a remarkable character, being eight or ten inches in length and only a quarter of an inch in diameter, preserving a tolerably perfect hexagonal shape throughout the entire length; again, these slender forms are built up of small hexagonal prisms, their faces projecting

from the side. It sometimes happens that these slender crystals are crossed by one of the same diameter, and less length, firmly attached in the manner of a cross.

But of all remarkable crystallizations is one, where the small prisms are so arranged as to form a perfect double spiral arranged around an axis, (fig. 3); the specimen is three inches in length

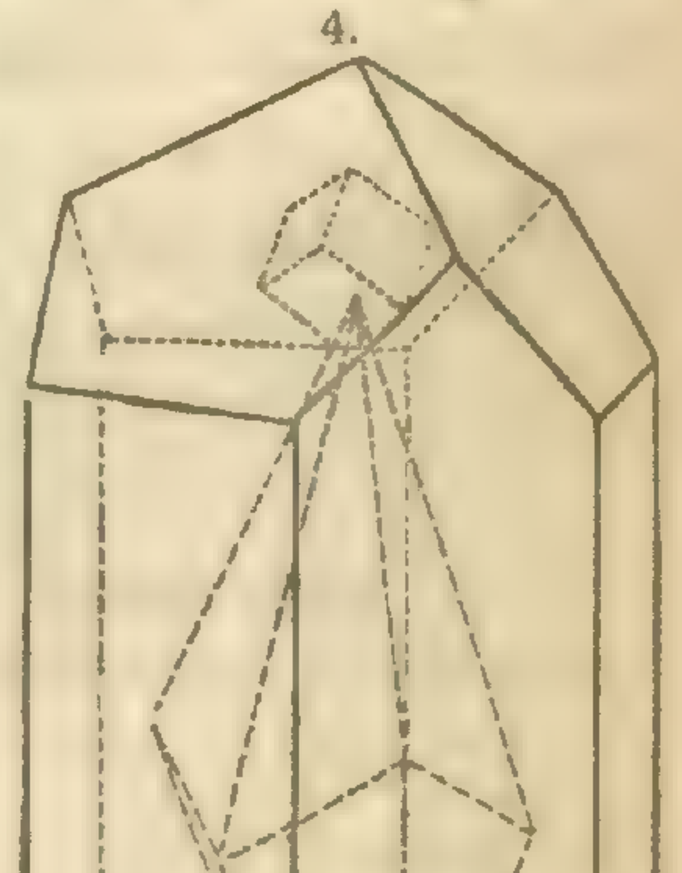
2.



and $\frac{3}{8}$ths of an inch in diameter, with the space of a $\frac{1}{4}$th of an inch between each turn of the spiral. The spiral arises from one small prism crossing another at middle at a small angle of divergence (40°–50°), and so on in succession. These slender crystals are sometimes curved in a very remarkable manner.

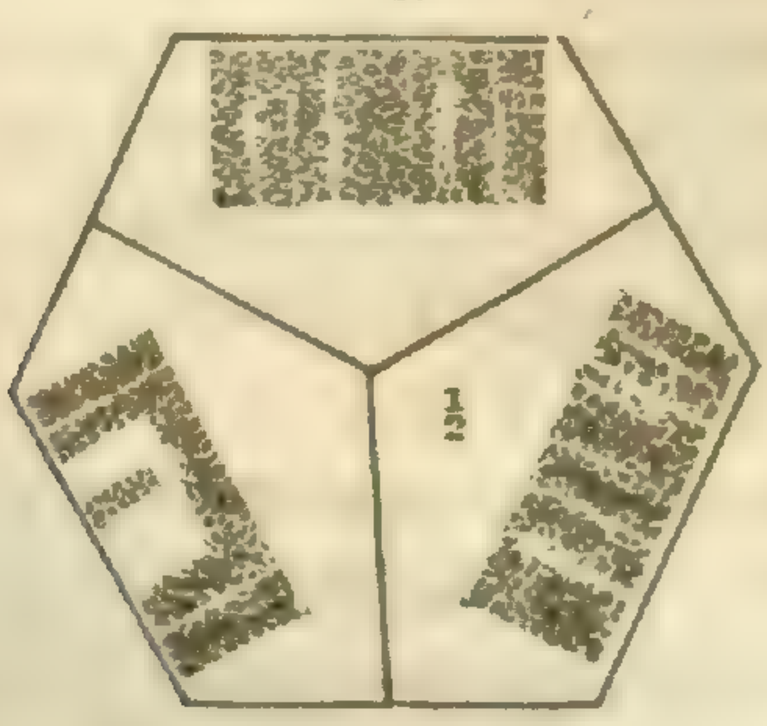
Another thing to be remarked in connection with the calcite of this mine, is its singular associations; thus, we find groups of hexagonal prisms where a small cubical crystal of fluor, about the $\frac{1}{20}$th of an inch, is inserted in a small pit in the summit of almost every crystal (fig. 4) without the occurrence of fluor spar on any other parts of the crystal. These crystals appear to have been formed by successive crystallizations. Dog-tooth spar seems to have been first formed with these small crystals of fluor spar on their extremities, and then by a subsequent process, the calcite has closed around the dog-tooth spar in the form of a hexagonal prism with a three-sided summit. The summit never closes entirely at the centre, the fluor spar remaining visible on one side, and where there is no crystal of fluor spar the extremity of the dog-tooth spar is frequently seen.

4.



Other groups of calcite crystals, have minute crystals of iron pyrites in the three faces of the summit, arranged near and per-

5.



6.

fectly parallel to the alternate edges as seen in figure 5. Every crystal in the group is thus furnished with a set of crystals of pyrites.

In another group of crystals the pyrites in equally small crystals is found in three lines on the summit of every crystal running from the apex towards the edges, exactly bisecting each face as seen in figure 6.

In this instance, as well as in the former, the pyrites is inserted entirely beneath the surface of the crystal, which is perfectly smooth.

The calcite is found in large crystals in dolomite, and is associated with most of the ores of the mine. It sometimes gives rise to pseudomorphs of molybdate of lead and carbonate of lead; these pseudomorphs are mere shells however, retaining the form of the calcite.

64. *Sulphur.*

Sulphur occurs in the form of small pale greenish yellow crystals; they are transparent and disseminated through cellular galena which appears to have undergone partial decomposition; the galena in which it occurs is frequently associated with copper and iron pyrites and in some rare instances with carbonate and phosphate of lead.

The other minerals occurring in the Wheatley mine are finely crystallized *quartz*, *oxyd of manganese*, *iron pyrites*, *sulphate of baryta*, *indigo copper*, *black oxyd of copper*, and *dolomite*.

Of the other mineral veins in this region, none have yielded the beautiful mineral species furnished by the Wheatley vein. The Perkiomen vein, five miles from the Wheatley vein, has furnished fine *capillary copper*, *indigo copper*, fine acicular crystals of *sulphate of baryta*, *crystallized copper*, and some crystals of *sulphate*, *carbonate* and yellow *molybdate of lead*, but these last were small and bear no comparison to those described.

It was hoped that something might be learned concerning the formation of the minerals of this vein, but the difficulties and uncertainty attendant upon the study of questions of this kind make it prudent to postpone any views that might be suggested. It may, however, be well to remark that in opening the vein and descending from the surface for the first thirty feet, the phosphate of lead was very abundant with some galena and carbonate; a little lower down the phosphate was less, and the carbonate more abundant. Wulfenite and anglesite began to appear at 120 feet, the phosphate and carbonate still continued with the galena, with fine large crystals of anglesite and considerable wulfenite; at 180 feet, phosphate very much diminished, carbonate and sulphate in fine crystals; arsenate was found here; at 240 feet, blende, calamine and fluor spar appear with considerable dolomite and but little phosphate of lead, galena forming almost the whole lead ore, anglesite is found, but in smaller crystals. These observations may hereafter lead to some conclusions as to the manner of the formation of these minerals, but at present I prefer dismissing the subject without further remarks.

Art. XXV.—*On the Meteorology of Oroomiah;* by Rev. D. T. Stoddard.*

The village of Seir, where we reside, is in the province of Oroomiah, in Northern Persia, in latitude 37° 28′ 18″ north, and in approximate longitude 45° east from Greenwich. We are about 40 miles from the boundary of Turkey and 150 from that of Russia. The village is on the grassy slope of a mountain, which rises 2834 feet above the neighboring city of Oroomiah and 7334 above the ocean. The side of the mountain on which we live faces the northeast, and is consequently somewhat bleak in winter. The snow also lies upon it in the spring long after it has disappeared from the southwestern side.

From the village of Seir we look down on the very beautiful and extensive plain of Oroomiah, forty miles in length and from twelve to twenty miles in breadth, which possesses a deep alluvial soil and bears on its fertile bosom several hundred villages. The city of Oroomiah, the ancient Thebarma, situated near the centre of the plain, as well as many of the villages, is surrounded by innumerable gardens and orchards, and rows of poplars, willows and sycamores, which make large portions of the plain resemble a continued forest. The mountains of Koordistan encircle the plain on three sides, while to the east lies the lake of Oroomiah, studded with islands and reflecting the pure azure of an Italian sky.

This plain is watered by three rivers of moderate size, which come down from the Koordish mountains, and are distributed by a network of small canals and water courses over its whole surface. Without artificial irrigation but few crops can be brought to maturity, although here and there wheat fields are cultivated on the slopes of the neighboring mountains, which are wholly dependent on the rains of the spring and early summer, and sometimes yield a tolerable harvest.

The principal productions of the plain of Oroomiah, the annual mean temperature of which is of course considerably above that of Seir, are wheat, barley, corn, millet, flax, tobacco, rice, cotton, castor oil, apples, pears, plums, grapes, (which are cultivated in immense vineyards) cherries, apricots, nectarines, peaches, melons, pomegranates, almonds and the jujube. The fig, with care, may also be cultivated, but is often destroyed by the cold of winter.

The lake of Oroomiah, the ancient Spautes, is about ninety miles long by thirty broad†. Its water has been thus analyzed by President Hitchcock:

* From a letter to Professor D. Olmsted, dated Seir, Oroomiah, January, 8, 1855.
† Its elevation above the ocean is 4100 feet.

In five hundred grains.	Grains.
Chlorid of calcium,	0·74
Chlorid of manganese,	5·76
Chlorid of sodium,	90 58
Sulphate of soda,	5·67
	102·75*

In the water of this lake it is impossible for a man to sink, the specific gravity being 1·155, and those who bathe in it come out encrusted with salt. In the summer, its shores are also fringed with a broad, white margin of salt, produced by evaporation. The lake exerts of course a marked influence on the climate of this region, and, as will be seen from the tables, produces a regular land and sea breeze in the summer months. During the day a light wind blows from the lake, aad during the night a fresher wind from the lofty mountains of Koordistan, which rise some forty miles west of the lake, to the height of 10 or 12 or perhaps 13 thousand feet above the ocean, and generally retain on their summits, even in summer, deep masses of snow. The amount of watery vapor is thus probably much greater in Oroomiah than in many parts of Persia, which present almost the barrenness of the Arabian deserts.

It should be mentioned in this connection that all the mountains in Northern Persia are destitute of trees, and many of them rise to a great height in naked rocky summits. Indeed, in the valleys and on the plains, it is rare to find any trees, except those planted by the hand of man, and a stranger, as he looks down on the luxuriant plain of Oroomiah, can hardly be made to believe that the millions of trees before him are entirely an artificial growth.

I regret that it has not been convenient for me to take *hourly* observations on the temperature, at least so far as to fix the proper times of observation for this region. Hourly observations, which have been made in countries remote from Persia and in climates entirely different from this, on the basis of which the tables of Kaemtz and others are founded, I have supposed would avail little for my purpose. The hours selected were those which suited best my convenience. I also desired to procure the extremes of temperature, which are here almost uniformly just before sunrise and at two or three in the afternoon. Of course, the average of the tables is below the actual mean temperature, though I am unable to say how much. It may however be interesting to mention, that a very large spring, just above our house on the mountain side, maintains throughout the year the temperature of 53° Fahrenheit. I have never found, summer or winter, a variation of more than half a degree.

* See an article in the Transactions of the Association of American Geologists and Naturalists.

1. *Thermometer and Wet Bulb Hygrometer.*

1852.	Monthly means of Thermometer. Sunrise	2 P. M.	10 P. M.	Mean.	Monthly means of Hygrometer. unrise	2 P. M.	10 P. M.	Mean.	Diff. between Therm. & Hygr. Sunrise	2 P. M.	10 P. M.	Mean.	Therm. highest	Date.	Therm. lowest.	Date.	Diff.	Greatest change in twenty-four hours.
April,	40°·8	54°·4	44°·50	46° 56	38°·9	46°·6	40°·	41°·8	1°·9	7° 8	4°·5	4°·76	65°	22d & 24th	30°	15th	35°	20°, 2d.
May,	52°·45	65°·5	56°·90	58° 28	45° 8	54° 07	47°·13	49°	6°·65	11°·43	9° 77	9° 28	77°	12th	41°·5	4th	35°·5	19°, 8th.
June,	57°·1	72°·6	62°·70	64°·13	49° 9	58° 6	52°·7	53°·7	7°·2	14°	10°	10° 43	78°	16th	52°	5th	26°	23°, 5th,
July,	65°·5	78°·8	70°·14	71°·48	54°·9	61°·45	56°·3	57° 5	10° 6	17°·35	13°·84	13°·98	86°·5	25th	59°	10th	27°·5	20°, 8th & 9th.
August,	67°·4	79°·4	71°·37	72° 72	54°·8	60°·4	55° 4	56°·9	12°·6	19°·00	15°·97	15° 82	89°	2d	58°	31st	31°	18°, 4th & 5th.
September	58°·3	70°·9	63°·17	64°·12	50° 68	57°·05	52°·2	53°·31	7°·62	13°·85	10°·97	10°·81	76°	2d	52°	30th	24°	17,° 2d.
October,	48°·7	61°·3	53°	54°·33	43°·6	50°·4	45°·5	46°·5	5°·1	10°·9	7°·5	7°·83	65°·5	6th	44°	26th	21°·5	18°, 16th & 17th.
November.	38°·1	47°·7	40°·80	42°·20	37°	42°	37°·7	38°·9	1°·1	5°·7	3°·1	3°·3	59°	2d	29°	20th	30°	15°·5, 9th & 10th.
December,	30°·7	38°	32°	33° 56	.....	36°·6	.....	.....	.....	1°·4	.....	....	43°·5	4th	21°	30th	22°·5	15°, 26th & 27th.
Jan. 1853,	24°	33°·3	26°·20	27°·83	.....	.....	.....	.....	.....	.....	.....	.....	41°	24th	17°·5	28th	23°·5	18°, 25th & 26th.
February,	28°·8	39°	30°	32° 60	.....	.....	.....	.....	.....	.....	.....	.....	58°	26th	12°	10th	16°	14°·5, 15th & 16th.
March,	35°·9	48°·1	39°·10	41° 02	.....	42°·6	.....	.....	.....	5°·5	.....	.....	57°·5	22d	23°	15th	34°·5	25°, 27th & 28th.
Annual mean,	45°·64	57°·42	49°·15	50°·73														
April,	42°·3	55°	45° 8	47°·7	39°·1	48°·2	42°·5	43°·3	3°·2	6°·8	3°·5	4°·4	64°	26th	32°	1st	32°	19°·5, 27th & 28th.
May,	48°	60°·6	52°·1	53°·5	45°·2	53°·1	48°·2	48°·8	2°·8	7°·5	3°·9	4°·7	70°	24th	42°	3d	28°	21°, 23d.
June,	57°·2	72°·2	62°·3	63°·9	51°·2	59°·5	54°·3	55°	6°·	12°·7	8°	8°·9	80°	27th	49°	1st	31°	21°, 27th & 28th.
July,	63°·4	75°·6	68° 2	69°·1	56°·2	61°·7	57°·8	58°·5	7°·2	13°·9	10°·4	10°·6	84°	24th	56°	10th	28°	15°·5. 8th & 9th.
August,	67° 6	79°·1	71° 6	72°·7	57° 9	63°·8	59°·1	60°·3	9°·7	15°·3	12°·5	12°·4	83°	16th, 20th	62°	4th	21°	15°, 10th.
September	60° 9	69°·4	63°·9	64°·7	54°·6	59° 7	56°·4	56°·9	6°·3	9°·7	7°·5	7° 8	79°	1st	56°	17th	23°	16°, 11th & 12th.
October,	50°·4	58°·8	53°	54°·1	17°·2	52°·5	48°·9	49°·5	3°·2	6° 3	4°·1	4°·6	67°·5	4th	33°	29th	34°·5	14°, 9th.
November,	37°·1	46°·4	39°·1	40°·8	36°·5	43°·2	37°·5	39°·1	0°·6	3°·2	1°·6	1°·7	54°	22d	30°·5	6th	24°·5	14°, 13th, 14th, 15th.
December,	33°·1	39°·6	34°·9	35°·8	.....	37° 6	.....	.....	.....	2°	.....	.....	48°	29th	27°	9th	21°	10°, repeatedly.
Jan. 1854,	24°·7	30°·7	26°·1	27°·2	.....	.....	.....	.....	.....	.....	.....	.....	43°	1st	10°	30th	33°	13°, 17th & 18th.
February,	29°	37°·7	31°·1	32°·6	....	.....	.....	.....	.....	.....	.....	.....	45°	20th, 22d	11°·5	7th	33°·5	17°, 14th & 15th.
March,	29°·3	39°·3	31°·5	33° 3	.....	.....	.....	. ...	.....	.....	.....	.....	49°	24th, 25th	9°·75	9th	39°·25	19° 5, 8th & 9th.
Annual mean,	45°·25	55°·36	48°·3	49°·61														
Mean for 2 years,	45°·44	56°·39	48°·72	50°·17														

N. B. The wet bulb was so often frozen during the winter months that the observations were irregular.

1852.	Monthly means of Barometer.				Baroin. highest	Date.	Baroin. lowest	Date.	Diff	Clouds, 0 – 10.				Cloud's days.	Rain and melt'd snow
	Sunrise	2 P. M	10 P. M	Mean.						Sunrise	2 P. M.	10 P. M.	Mean.		
April,	24·177	24·168	24 187	24·177	24·305	20th & 21st,	23·897	27th & 28th,	·488	5·1	6·5	4·3	5·3	0	
May,	24 283	24·301	24·304	24 296	24·421	13th,	24·085	2d.	·336	3 1	4·3	2·	3 1	3	
June,	24·225	24 211	24·211	24·216	24·341	2d,	24·094	19th,	·247	Observations imperfect.					
July,	24·193	24·193	24·189	24·192	24·303	1st,	24·049	8th,	·254	·04	·03	·01	·03	26	
August,	24·246	24·247	24·235	24·242	24 417	26th,	24 097	4th,	·320	.20	·20	·30	·23	20	
September,	24·330	24·325	24·322	24·325	24·45	30th,	24·18	19th,	·270	1·0	1·4	1·8	1·4	12	
October,	24·444	24·433	24·442	24 439	24·53	7th,	24·30	17th,	·230	1·4	2 9	1 0	1·7	6	
November,	24·336	24·333	24·331	24 334	24·47	25th,	24·21	7th,	·260	3·2	5 1	4·8	4 3	3	
December,	24·350	24·361	24·350	24·353	24·59	13th,	24·22	11th,	·370	3·9	5·1	3 8	4·3	2	
January, 1853,	24·330	24·325	24·329	24·328	24·47	19th & 20th,	24·18	23d,	·290	3·8	3·8	3·8	3·8	2	
February,	24 290	24·300	24 313	24·301	24·60	20th,	23·97	6th,	·630	4·7	3·8	4·6	4·4	1	1·42
March,	24·240	24·217	24 324	24·230	24·51	3d,	23·91	14th,	·600	3 6	5·7	3·4	4·2	0	1·41
Annual mean,	24·287	24 284	24·287	24·286						No. of cloudless days, -				75	
April,	24 230	24·242	24·245	24·239	24·40	30th,	24·05	8th,	·35	3·1	5·2	3·	3·8	0	5 22
May,	24·299	24·292	24·297	24·295	24·47	12th,	24·15	4th, 5th & 20th,	·32	2 9	5·3	4·1	4·1	1	2·43
June,	24·219	24 207	24 214	24·213	24·36	1st & 15th,	24·05	29th,	·31	0·9	1·8	2·2	1·6	8	·43
July,	24·219	24·205	24·203	24 209	24·37	18th,	24·12	24th,	·25	1·0	1·2	0·6	0·9	12	·00
August,	24·290	24 285	24·278	24·284	24·38	22d,	24·14	8th,	·24	1 5	1·8	1·1	1·5	6	·48
September,	24·292	24·279	24·277	24·283	24·37	5th, 6th, &c.,	24·05	11th,	·32	1 3	2·3	1·6	1·7	4	·65
October,	24·417	24 411	24·415	24·414	24·53	28th,	24·19	27th,	·34	2 5	4·5	3·2	3·4	1	1·48
November,	24·335	24·328	24·335	24·333	24·49	16th,	24·17	21st,	·32	4·4	5 9	4·	4 7	2	·95
December,	24·305	24·294	24·297	24·298	24·42	23d & 24th,	24·08	6th,	·34	5.	6·2	5·1	5·4	1	1·29
January. 1854,	24 280	24·264	24·270	24·271	24·49	10th & 11th,	24·02	31st,	·47	5.2	4·3	3·2	4·2	4	1 69
February,	24 074	24·078	24·089	24·080	24·41	21st,	23·79	17th,	·62	4·5	6·1	4·7	5·1	1	4·25
March,	24·133	24 134	24·146	24 137	24·44	3d,	23·88	1st,	·56	3·4	6·4	2·9	4·2	0	6·71
Annual mean,	24·258	24·251	21·255	24·255						Am't of rain during the year,					25·58 in.
Mean for two years,	24·272	24·267	24·271	24·271						No. of cloudless days,				40	

Barometer highest during the two years, 24·60, February 20th, 1853.
" lowest " " " 23·79, February 17th, 1854.
Extreme range, - - - - - - ·81.

Amount of rain rnd melted snow for the year ending December 31st, 1855, was 21·62 inches.

As a general rule I have been very systematic and regular in taking the observations and have neglected to record but few each month, excepting those on the winds. I think therefore, if the results are erroneous, it is rather owing to a defect of the instruments than to a want of faithfulness in using them. In regard to the barometer, I have sometimes suspected it was injured in the transportation from America, but my doubts were in a measure removed by finding that it agreed with the Aneroid Barometer of Chevalier Khanikoff and that the measurements of the height of Oroomiah above the ocean are nearly like those of previous observers. The barometrical records are carefully corrected for temperature being reduced to the foregoing point; but, as I did not know the bore of the tube, I could not make the correction for capillarity. The diameter of the bore must however vary from $\frac{1}{6}$th to $\frac{1}{7}$th of an inch. All the instruments have been in a favorable position and about ten feet from the ground.

In one respect, at least, my labor is quite incomplete. As the tables now stand, some *general* notion may of course be formed of the tension of vapor, by noting the difference of the thermometer and hygrometer. But, as the barometrical pressure is always to be taken into account, and the pressure at this altitude above the ocean is much less than in the United States, a general inspection of the tables might lead the reader to very erroneous conclusions as to the dryness of this climate. I should have reduced these observations, if I had had the means of doing so conveniently. I leave this for those who may be interested in the results.

Art. XXVI.—*Correspondence of M. Jerome Nicklès, dated Paris, June 29, 1855.*

Aluminium and Sodium.—Aluminium has already been introduced into the industrial arts. In the session before the last of the Academy of Sciences, M. Dumas exhibited in behalf of M. Deville, large masses of the chlorid of aluminium, and of the metals sodium and aluminium, obtained at the chemical works of Javel. Dumas observed that two to three hundred kilogrammes of chlorid of aluminium had been already made, showing that it may become a material of manufacture on the large scale.

It has been proved that sodium, while superior in energy to potassium, may be prepared by Deville's process, with no difficulties not incidental to the manufacture of the latter metal. Numerous trials have also shown that it may be kept in fusion in contact with the air without inflaming, and that it may be run out of the apparatus which furnishes it. A metal like sodium, brought within the reach of science and the arts, must soon come into extensive use.

M. Dumas also remarked on the fact that the study of aluminium had introduced a new process into the arts for the reduction of ores—that from the chlorid of the metal; and that this method might be important for other metals not yet brought into use. He also mentions the sonorousness of aluminium, a quality in which it compares with the best bronze, having a quality of tone not hitherto observed in any metal in the pure state, which is another peculiarity of this curious metal.

He stated, in reply to inquiries, that the materials employed in making 1 kil. of aluminium—viz., the ammoniacal alum, the alumina which is derived from it, chlorine, carbon, carbonate of soda, chalk, are all of low price. The whole cost is reduced to 32 francs, which is very small, when we consider that the expense of sodium, when the experiments in aluminium were begun, was 1000 francs per kilogram, which alone would make the price of aluminium 3000 francs. M. Balard, who is familiar with industrial applications, stated that he had gone through with the steps of the process at Javel, and was satisfied that the cost of the sodium could be still further reduced when prepared on a large scale.

The chlorid of aluminium is prepared at the Javel works by the reaction of chlorine on a mixture of alumina and coal tar previously calcined—which is easily effected in a gas retort. The condensation of the chlorid is produced in a chamber of masonry, lined with earthen ware. The chlorid contains a little iron, which is removed entirely in treating it for aluminium, by making its vapor to pass over points of iron heated to 400° C. The sesquichlorid of iron, $Fe^2 Cl^3$, as volatile as the chlorid of aluminium, is changed, through contact with iron, into the protochlorid, Fe Cl,—which is relatively very fixed. The vapor of the chlorid of aluminium, on leaving the apparatus, affords colorless transparent crystals.

In preparing the sodium, there are used—

Dry carbonate of soda,	1000 parts.
Chalk,	150 "
Dry coal of Charleroy,	450 "

which are pulverised, mixed with care, and calcined to a red heat in a pot.

Wheatstone long since showed that aluminium was as strongly negative as platinum. M. Hulot, director of the galvanizing works and coinage of Paris, has tried the use of the impure aluminium in a galvanoplastic battery. He has found that a couple of aluminium and zinc, the latter amalgamated some considerable time previously, when charged with water acidulated with a twentieth of sulphuric acid at 66° C., has afforded during the first hours a current at least equal to that from a couple of platinum and zinc, excited to the same degree. After six hours, the current had lost a fifth of its original force. The battery was not completely polarised at the end of 24 hours, and the current still preserved a quarter of its first force. To restore its electro-negative character to the aluminium, it was necessary only to immerse it an instant in nitric acid and then wash it.

According to the Messieurs Tissiers, pure aluminium is easily distinguished from the impure, by its greater whiteness, its indistinct traces of crystallization, and rarely one or two well defined hexagons on the

surface of the ingots, while the impure has a bluish tint like zinc, and if the whole is not crystalline the upper surface is much more so than in pure aluminium, and the form is also quite different. According to one of the most extensive galvanising establishments of Paris, the metal works as well as silver.

It may be whitened easily by dipping the piece in a concentrated solution of soda or potash, and passing it then into nitric acid. This acid acts differently according as it is itself pure or mixed with chlorhydric acid, and according as the aluminium is pure or not.

Aluminium is most easily soldered when alloys are used containing aluminium. The alloys most convenient are those with silver, zinc or tin; the point of fusion being below that of aluminium, the soldering may be done by means of a simple spirit lamp, and without any previous cleaning.

Fulminating power of Silver in the state of Sponge.—Correction.—The sponge of silver and not of silicium possesses the property of fulminating under a heavy pressure suddenly applied—a property on which a remark was made in the last number in connection with the observations of M. Chenot.

Physiological and Therapeutic effects of Carbonic Acid.—Some weeks since M. Herpin, of Metz, stated the following facts to the Academy. Dr. Struve took the Marienbad waters for a painful affection of the leg. He had been unable for several weeks to walk without a crutch. Dr. Struve had the notion one day of exposing his leg to the action of the carbonic acid which escapes freely from the spring at Marienbad, and forms a bed several decimeters deep over its surface. After exposure for a while it produced a pricking sensation and warmth, which went on increasing till it occasioned a profuse perspiration of the diseased limb. On withdrawing then his leg, he was surprised to feel no pain, and to find that he was able to walk without his crutch. He continued this treatment for some time, and has since experienced no return of his complaint.

There are now in Germany special establishments for baths, douches, and the inhalation of carbonic acid. According to M. Herpin, the first effect of exposure to the gas is a sensation of pleasant heat, like that from a garment of fine wool—then a pricking, and afterwards a burning which has been compared to that from mustard.

On the occasion of this communication, M. Boussingault related how he happened in 1826 to witness some of the effects mentioned by M. Herpin. He was visiting in the Quindiu, New Grenada, a part of the Cordillera, some *solfataras*, where numerous workmen were occupied melting and purifying sulphur. He encountered a crevice whence issued abundantly a gas of the odor of sulphuretted hydrogen. He attempted to descend in it in order to ascertain the temperature; but he had hardly entered the crevice when he felt a suffocating heat, which he estimated to be at 40° C., and a pricking in the eyes; respiration being difficult, he ascended quickly; his face was red and his perspiration abundant. After a while he descended again with his thermometer, and was surprised to find a temperature of only 19½° C. The extreme temperature was 22° C. The gas was composed of 95 per cent. of carbonic acid and 5 p. c. of atmospheric air and sulphuretted hydro-

gen. It was hence the carbonic acid which caused the sensation of heat and the irritation of the eyes. At two other times, in 1827 and 1830, M. Boussingault experienced again the same sensations. He observed that the workmen who work long in the solfataras of the Cordillera, in contact with the carbonic acid, experience an enfeebling of the sight, and some of them become blind.

Dr. Herpin confirmed the fact with regard to the action on the eyes of the baths of gaseous carbonic acid. He observed that the douches of carbonic acid had been used against "*amblyopie*" or enfeebling of vision, and different precautions were used to moderate the effects of the gas or diminishing the force of the jet, or its distance; or its direct action by interposing muslin. When the eyes have an inflammatory tendency, it irritates the organ and even the neighboring parts; the heat sometimes produces dangerous congestion.

Thermogenic Apparatus.—For some time past there has been a machine at work on the Quai Valony, at Paris, which furnishes a considerable quantity of steam without any other source than that of friction. The machine consists of a cylindrical heater two meters long, 50 centimeters in diameter, having through all its length, within at centre, a conical tube. The water which is reduced to vapor fills the void space between the inner walls of the heater and the outer of the conical tube. Into the conical tube is passed a cone of wood, covered throughout with a braid of hemp rolled spirally. The cone of wood is traversed by an iron axis, and fills exactly the exterior capacity of the tube, so as to rub constantly against its walls. It is put in motion by a fall of water from the Canal St. Martin, so as to make about 400 turns per minute. The heat from the friction is sufficient to turn to steam the water of the boiler; a thermometer placed within the boiler indicates at the end of a certain time a temperature of 130° C. The boiler is strengthened in the ordinary way, with safety stop-cocks, a float, manometer, &c. The vapor reaches hardly a pressure of $2\frac{1}{2}$ atmospheres. A greasing apparatus conveys constantly to the envelop of the cone the oil required to sustain the motion.

This machine holds 400 litres of water. To be set in action it requires the power of two horses, and then produces steam enough to do the work of one horse. The authors, MM. Beaumont and Major, hope thus to utilize the force of falling water and convert it into heat. This machine is at work at the Crystal Palace of Paris.

Gas from Peat.—There is much discussion in connection with the renewal of the engagements of the city of Paris with the gas companies. Attention has thus been called to the gas from peat, which for some time has been manufactured at Paris. M. Leon Foucault has been charged with measuring the comparative illuminating powers of coal and peat gas; and the result is in favor of that from peat, its power being 342, while that of coal gas is 100.

The manufacture of peat gas is more simple than that of coal. The peat, if put into an iron retort, heated to a low red heat, affords immediately a mixture of permanent gases, and vapors which condense into an oleaginous liquid, which two products separate on cooling. The oil is collected in a special vessel, and the gas passes into a gasometer. This carburetted hydrogen is wholly unfit for illumination, it.

giving a very small flame, nearly like that from brandy. The oil from the peat is a viscous blackish liquid, of strong odor; it is subjected to a new distillation, and resolved wholly into a permanent gas and hydrogen very richly carburetted. This mixture is strongly illuminating, giving a flame six or eight times brighter than the first and of more lively brilliancy. The two are mixed, and a gas of intermediate character obtained, which is delivered over for consumption.

M. Foucault has made his trials with a photometric method which will soon be made known. Its unit was not a single wax-candle, but a collection of seven candles, arranged in a hexagonal manner with spaces of one centimeter. A single candle is liable to too much variation, a compensation for which is secured when a number is employed.

By this method, a mean of five determinations gave for a burner of peat gas a light equivalent to $23\frac{1}{4}$ candles; and the same burner with coal gas $6\frac{3}{10}$ candles.

The illuminating power of the pure oil from peat—the illuminating material "par excellence"—has been found, at equal pressures, 705, the intensity for coal gas being 100; and with equal volumes their numbers are as 756 : 100.

Practical Acoustics; Progressive elevation of the diapason of Orchestras.—At the commencement of the 18th century, during the later years of the reign of Louis XIV, the *la* adopted in the orchestra corresponded, according to Sauveur, to 810 vibrations per second; the number now for the *la* of the opera is 898. There has hence been, from 1715 to 1855, a rise of nearly a tone in the diapason of the orchestra. This rise has taken place mainly in the present century, and most rapidly in the last 25 years. The following facts will give some idea of the change.

(1) The *la* of the Royal Chapel under Louis XVI. corresponded to 818 vibrations; (2) in 1808, the *la* of a flute of Holzappel, as estimated by Delezenne, was 853; other diapasons of the same epoch give 857 or 860 vibrations; (3) in 1823, at the Italian Theatre, it was 848, 855 at the Feydeau theatre, 863 at the Opera, 872 at the Conservatoire; (4) in 1834 the *la* at the Opera corresponded to $867\frac{1}{2}$ vibrations, at the Conservatoire 870; (5) in 1855, finally, the *la* of the operas is 898 vibrations.

Thus since 1823 the rise has been nearly a semitone; and it is therefore not astonishing that tenor voices should be so rare.

This rise in the diapason has not reached its limit. In fact, since wind instruments have had great importance in the orchestra, they have necessarily, in consequence of their sonorousness, imposed their tone on stringed instruments. But the diapason of these instruments tends always to rise. This takes place principally through military music, for there the rise of tone has some advantages, it increasing its sonorousness, its acuteness of tone, and therefore its piercing character, and on the other hand diminishing the weight of the instrument. The makers of instruments incline rather to diminish the size, and thereby to raise instead of depressing the diapason.

The manufacturers of pianos have the same tendencies. To obtain the fullest sonorousness, it is necessary to give the strings a tension very near that which is necessary to break them; and consequently, as the

cords are improved in quality, they tend more and more to raise the tone, especially as the instrument admits of this increase of tension without changing its model.

There is also a permanent cause for the rise of the diapason in the method used for tuning. In fact, it is done by the file; and filing a diapason heats it: at the moment it is in tune with the standard, it is thus heated; and when it cools afterwards the tone will rise. When this second diapason is used for tuning, there is another similar rise, and so it continues.

These considerations have led M. Lissajous, Professor of Physics at the College of St. Louis, at Paris, to propose the adoption of a uniform diapason, and means for preserving it. He desires that the question should be settled by an international congress. Without wishing in advance to fix the number to be employed for the normal diapason, he suggests that 1000 vibrations should be taken for the *si* natural of the mean scale of the piano; the *la* corresponding, will then, in the system of equal temperament, correspond to 890 vibrations, which is very near the actual *la* of the Conservatoire, or the mean *la* adopted by the manufacturer. This method will have the advantage of connecting indirectly the scale of sound with the decimal system.

Zincography.—An engraver at Paris, M. Dumont, has just communicated to the Société d'Encouragement a process for electric engraving, which promises much. On a thick plate of zinc, grained with a mallet of steel and fine sand, he designs some subject with a kind of lithographic crayon, and on this he spreads a fine powder made of a mixture of resin, Burgundy pitch and asphaltum: on heating the zinc plate, the powder melts and forms a varnish which spreads over the parts of the surface which have been covered with the lithographic crayon, that is, over the whole design. To obtain the design in relief, he then plunges the plate into a bath of sulphate of zinc in communication with the positive pole of a battery in front of another plate connected with the negative pole. The current passes and corrodes part of the zinc not covered with ink, and the picture comes out in relief. From the plate thus engraved in relief a cast is taken in gutta percha, in which copper is deposited in order to obtain the engraved plate, from which impressions may be had by means of the ordinary typographic press.

On Horseflesh for food.—M. I. Geoffroy St. Hilaire, the President of the Zoological Society for Acclimation and Professor at the Mus. d'Hist. Naturelle, &c., in a course of lectures on animals useful to man, has just devoted two lectures demonstrating the advantages of horseflesh for food. After speaking of the predilection of the ancient Germans for horseflesh, he has inquired into the aversion now so general. Both the Scandinavians and Germans, kept in a sacred pasture a race of white horses for sacrifices to Odin, and after the sacrifice they boiled them for a feast. This is no doubt the origin of the hippophagy which continued among these people until driven out as a part of paganism by the spread of Christianity. Yet in spite of the efforts of Pope Gregory III. and his successors, the use of horseflesh continued for a long time in Scandinavia. The race of white horses is still found pure in the stables of Fredericksberg belonging to the King of Denmark.

The nomadic tribes of Asia retain still their relish for horseflesh although they have an abundance of cattle and sheep.

Among the people of Europe, who have anew taken up the use of horseflesh, the Danes were the first. During the siege at Copenhagen in 1807, it was authorized by the government, and since then it has continued to be eaten. In the Capital of Denmark, there is a butcher's stall for selling horseflesh alone, licensed by the government.

At times it has been introduced into Paris. During the scarcity at the time of the revolution, the greater part of the meat eaten for six months was from slaughtered horses, and no inconvenience resulted from it. Along the Rhine, in Catalonia, and in the maritime Alps, the celebrated Larrey many times had recourse to this article of food for his wounded soldiers. He depended on it mainly at the siege of Alexandria, and owed to it, in a great degree, the cure of the sick.

From these facts and multitude of others he enumerates, M. I. Geoffroy St. Hilaire concludes that horses may be used as wholesome, economical and nutritious food.

Bibliography—Traité Elémentaire de Physique par Péclet, Prof. á l'Ecole Céntrale des Arts et Manufactures, 4th edit., 2 vols., 8vo, Paris, Chez Hachette & C^ie^.—M. Péclet, is the author of the celebrated Treatise on heat, and better than almost any one knows to the bottom whatever concerns the imponderable fluids. The questions of practical physics are treated with the hand of a master, as well as theoretical questions. M. Péclet is the first French author who has done justice to M. Ohm in giving to him the priority in the discovery of the law of currents—a discovery which M. Pouillet had succeeded for some years in taking to himself.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the formation of alcohol from olefiant gas.*—Berthelot has succeeded in preparing alcohol by causing olefiant gas to unite indirectly with two equivalents of water. The discovery is particularly interesting because hitherto the alcohols have been formed exclusively by fermentation. Pure and previously boiled sulphuric acid by long agitation with olefiant gas slowly absorbed it. The acid on dilution with water and distillation then yielded alcohol. About 32 litres of olefiant gas and 900 gr. of sulphuric acid yielded in this way 52 gr. of alcohol, corresponding to 45 gr. of absolute alcohol. The process appears to depend on the formation of sulphovinic acid and its subsequent decomposition by heat, though the author thinks he has observed differences between the salts of the acid obtained in this way and those of the ordinary acid. Propylene C_6H_6 was found to be absorbed by sulphuric acid as readily as carbonic acid by potash. The liquid diluted with water and distilled, gave a mixture of water and propionic alcohol. Marked differences were observed between the characters of the salts of the acid having the constitution C_6H_7O, HO, $2SO_3$ ac-

cording to the circumstances and modes of preparation. In conclusion, the author states that when propylene gas is exposed to fuming muriatic acid it is slowly absorbed. The action takes place more readily at a higher temperature. The product is a neutral liquid insoluble in water and identical with the chlorid of propyl C_6H_7Cl.—*Ann. de Chimie et de Physique*, xliii, 385, *April*, 1855.

2. *On the theory of the Compounds of Glycerine*—The Lipyl theory of Berzelius has long since ceased to afford a satisfactory explanation of the chemical relations of glycerine, and the discoveries of Berthelot in particular, have rendered a new theory indispensible. Wurtz proposes to consider glycerine as a tribasic alcohol, that is, as containing 3 eqs. of hydrogen replaceable by 3 groups. Glycerine may be reduced to the type of $3(H_2O_2)$ if we suppose C_6H_5 capable of replacing 3 eqs. of hydrogen, then we shall have

$$\left.\begin{matrix}H\\H\\H\end{matrix}\right\}C_6H_5\left\{\begin{matrix}O_2\\O_2\\O_2\end{matrix}\right.$$

It is clear that this tribasic alcohol must yield 3 species of ether, since if R represents the radical of an acid, 1, 2 or 3 eqs. of hydrogen in the above formula may be replaced by 1, 2 or 3 eqs. of R. Thus the three species of stearine will have the formulas,

$$\left.\begin{matrix}C_{36}H_{35}O_2\\H\\H\end{matrix}\right\}C_6H_5\left\{\begin{matrix}O_2\\O_2\\O_2\end{matrix}\right. \qquad \left.\begin{matrix}C_{36}H_{35}O_2\\C_{36}H_{35}O_2\\H\end{matrix}\right\}C_6H_5\left\{\begin{matrix}O_2\\O_2\\O_2\end{matrix}\right. \qquad \left.\begin{matrix}C_{36}H_{35}O_2\\C_{36}H_{35}O_2\\C_{36}H_{35}O_2\end{matrix}\right\}C_6H_5\left\{\begin{matrix}O_2\\O_2\\O_2\end{matrix}\right.$$

Monostearine. Distearine. Tristearine.

while there will be three chlorhydric ethers having the formulas

$$\left.\begin{matrix}H\\H\\Cl\end{matrix}\right\}C_6H_5\left\{\begin{matrix}O_2\\O_2\end{matrix}\right. \qquad \left.\begin{matrix}H\\Cl\\Cl\end{matrix}\right\}C_6H_5\left\{O_2\right. \qquad \left.\begin{matrix}Cl\\Cl\\Cl\end{matrix}\right\}C_6H_5$$

Monochlorhydrine. Dichlorhydrine. Trichlorhydrine.

All these combinations, as well as a triacid nitrate, a biacid sulphate and a uniacid phosphate, have been obtained already. As the radical C_6H_5 is tribasic and equivalent to 3 eqs. of hydrogen, we ought to expect an ether having the formula $\left.\begin{matrix}C_6H_5\\C_6H_5\\C_6H_5\end{matrix}\right\}O^2$ and this has been obtained by Berthelot and Luca. In conclusion, Wurtz points out that there ought to exist 3 glyceric ammonias, in which 1, 2, or 3 eqs. of NH may be supposed to replace 1, 2, or 3 eqs. of O_2, just as ethylamine may be considered as derived from alcohol by replacing O_2 in $C_4H_6O_2$ by NH.—*Ann de Chimie et de Physique*, xliii, 492.

[Note.—It is easy to see that all the above results may be equally well explained by employing for glycerine the formula $C_6H_5O_3+3HO$, and that it is not necessary to refer it to the type of water at all. Wurtz's view is based upon Gerhardt's theory that all acids may be considered as water with a single double or treble equivalent, a certain number of equivalents of hydrogen being replaced by other radicals. We may consider glycerine with the formula $C_6H_5O_3+3HO$, as hav-

ing the same relation to alcohol with the formula C_4H_5O+HO which SbO_3+3HO bears to $KO+HO$. The 3 stearines above mentioned will have on this view the formulas $C_6H_5O_3+C_{36}H_{35}O_3, 2HO$. $C_6H_5O_3+2C_{36}H_{35}O_3, HO$. $C_6H_5O_3+3C_{36}H_{35}O_3$; while the glycerine ether of Berthelot and Luca will be $C_6H_5O_3+C_6H_5O_3$, or more simply $C_6H_5O_3$, bearing the same relation to C_4H_5O which SbO_3 bears to ZnO. We ought to expect by the action of dehydrating agents upon glycerine to obtain the body $C_6H_4O_2$, since $C_6H_5O_3, 3HO-4HO=C_6H_4O_2$, and in like manner we should have the reactions $C_6H_5O_3, 3HO-5HO=C_6H_3O$, and $C_6H_5O_3, 3HO-6HO=C_6H_2$. Moreover we should have the acids $C_6H_6O_8$, and $C_6H_4O_{10}$, produced by successive substitutions of O_2 for H_2 just as acetic acid is produced from alcohol by a similar substitution.—W. G.]

3. *On some new bodies belonging to the Propionyl Series.*—ZININ has communicated the results of an investigation of the compounds of Propionyl which possess much interest and importance. It will be remembered that propionyl is the radical of propionic acid and is homologous with acetyl, its formula being C_6H_5. By the action of the iodid of this radical upon the salts of silver, Zinin has succeeded in preparing an entirely new class of ethers of which the acetate, benzoate and carbonate are described. Acetate of Propionyl $C_6H_5O+C_4H_3O_3$ (not to be confounded with acetate of propyl, $C_6H_7O+C_4H_3O_3$) is a colorless liquid, lighter than and but slightly soluble in water, but soluble in all proportions in alcohol and ether. It has a neutral reaction, a sharp odor like that of acetate of ethyl, and a sharp etherial taste: it boils at 150° C. Benzoate of propionyl is an oily liquid heavier than water and smelling like benzoic ether: its formula is $C_6H_5O+C_{14}H_5O_2$ and it boils at 242° C. Carbonate of propionyl is an oily etherial liquid lighter than water. The new ethers above described are all decomposed by contact with caustic potash either dry or in a concentrated aqueous solution. The decomposition is precisely similar to that of the corresponding ethyl compounds, the products being salts of potash and a volatile liquid soluble in water and having a faint odor but strongly attacking the lungs and eyes. The author does not give the constitution of this body, but it is doubtless the propionic aldehyde, the reaction in the case of the acetate being represented by the equation $C_6H_5O, C_4H_3O_3+KO, HO=KO, C_4H_3O_3+C_6H_5O, HO$.

The iodid of propionyl readily attacks mercury—much more easily than the iodids of methyl and ethyl. The product is the iodid of a new radical and has the formula $C_6H_5Hg_2I$; it is therefore analogous to ethyl-mercury, methyl-mercury, &c. By double decomposition with the salts of silver it gives well defined crystalline salts. Hence the analogy between propionyl and ethyl is clearly established.—*Bull. de St. Petersb.*, xiii, 360–363. *Chemisch. Pharm. Central Blatt., May*, 1855.

4. *On the artificial formation of Oil of Mustard.*—ZININ has made the interesting discovery that the volatile oil of mustard is the sulpho-cyanid of propionyl, and that it may easily be formed artificially by distilling an alcoholic solution of sulpho-cyanid of potassium with Berthelot's iodid of propionyl. (Am. Journal Sci., xix, 270). The oily fluid which separates from the distillate on the addition of water when

redistilled gives a liquid passing over between 145° and 150°, and possessing all the physical and chemical properties of the etherial oil of mustard. The reaction is represented by the equation $C_6H_5I + KCy S_2 = C_6H_5Cy S_2 + KI$.—*Bull. de Petersburg quoted in Journal für prakt. Chemie*, No 8, 504, 1855.

[NOTE.—It will be remembered that the oil of mustard has long been considered as the sulphocyanid of the radical allyl C_6H_5, and that the oil of garlic is a mixture of the oxyd and sulphid of the same radical. Zinin's discovery proves the identity of allyl and propionyl, and puts it in our power to obtain artificially the oil of garlic and the very curious compounds which it forms with metallic chlorids, sulphids, &c. Thus C_6H_5I distilled with a solution of KS should give C_6H_5S and KI. I will furthermore remark that the body having the constitution $C_{12}H_{11}I$, and which may be called iodid of capronyl, must give by distillation with RS the oil of assafetida, which in a pure state appears to be simply $C_{12}H_{11}S$.

Zinin's researches have done so much toward completing the theory of the compound radicals of the acetyl type, that it is worth while to examine in this place the parallelism which exists between the acetyl and ethyl series. To do this we shall place the types of the two series in a tabular form.

Ethyl,	C_4H_5	Acetyl,	C_4H_3
Ether,	C_4H_5O	Ac. oxyd,	C_4H_3O
Alcohol,	C_4H_5O, HO	Aldehyd,	C_4H_3O, HO
Chlorid of ethyl,	C_4H_5Cl	Chl. of ac.	C_4H_3Cl
Acetate of ethyl,	$C_4H_5O, C_4H_3O_3$	Acet. of ac.	$C_4H_3O, C_4H_3O_3$
Hydruret of ethyl,	C_4H_5, H	Olefiant gas	C_4H_3H,
Ethylamin,	$N\begin{cases} C_4H_5 \\ H \\ H \end{cases}$	Acetylamin,	$N\begin{cases} C_4H_3 \\ H \\ H \end{cases}$

Tersulphid of ethyl $C_4H_5S_3$
Anhydrous acetic acid $C_4H_3O_3$.

Acetic acid $C_4H_4O_4$, and the oil of the Dutch chemists $C_4H_4Cl_2$ have no analogues in the ethyl series.—W. G.]

5. *Preparation of Metallic Lithium.*—BUNSEN has continued his investigations on the alkaline and earthy metals and has succeeded in isolating lithium upon a sufficiently large scale to admit of an accurate determination of its chemical and physical properties. Lithium is more easily prepared than the other metals belonging to the same class, and its separation forms an easy and certain lecture room experiment. Chlorid of lithium is to be fused in a small thick porcelain crucible over a Berzelius lamp and decomposed by a current of from 4 to 6 Bunsen's elements. The current passes from a point of gas-carbon through the fused chlorid to an iron wire as thick as a knitting needle. After a few seconds a small silver-white fused regulus forms and adheres to the wire gaining in a few minutes the size of a small pea. The mass is to be removed from the fused chlorid by passing under it a small iron spoon, and withdrawing the spoon and the wire electrode together so that the metal shall remain covered with a varnish of the fused

chlorid. The spoon is then to be cooled under naphtha and the metal scraped off with a penknife. As these operations may be repeated every three minutes, an ounce of chlorid of lithium may be reduced in a short time. Lithium is a white metal having the color of silver, but a freshly cut surface presently becomes yellowish from oxydation. Fused at 180° and quickly pressed up between two glass surfaces, lithium gives a mirror which perfectly resembles polished silver in color and lustre: its streak upon the touchstone is gray while that of calcium, barium, or strontium is gold-yellow. During the oxydation of this last the yellow color passes for a moment into copper red; this is not the case with calcium and barium. Lithium is a very tough metal and can easily be drawn to a wire like lead, but its tenacity is much less than that of the latter metal. Lithium fuses at 180°, is not volatile at a red heat, and can be welded at ordinary temperatures. Its density is 0·5936, and it is the lightest of all solid bodies. If we take its equivalent as 81·7 (O=100) its atomic volume is 137. Lithium is less oxydizable than sodium and potassium; it takes fire at a temperature which is far above 180° C. and burns quietly without sparks and with a white and unusually intense light. The elevation of temperature is here so great that a piece of lithium weighing only 0·005 gr. will melt a hole of 36 millimetres in a piece of mica upon which it moves with a serpentine motion. Strontium and calcium burn in a similar manner, with a yellowish light however, and not so quietly, but with sparks and a hissing noise. Lithium burns in oxygen, chlorine, bromine, carbonic acid, vapor of iodine, and upon fused sulphur, like calcium and strontium, with extraordinary brilliancy and intense white light. Upon water lithium floats and oxydizes like sodium, but without fusing. Fuming or common nitric acid oxydises lithium so violently that it often fuses and takes fire. Calcium and strontium, on the other hand, are only attacked by dilute nitric acid, but scarcely at all by it when fuming and even boiling. Muriatic and dilute sulphuric acid dissolve lithium as well as calcium and strontium, with a violent evolution of gas; concentrated sulphuric acid has however very little action in the cold. Silica, glass, and porcelain, are reduced by lithium under 200° C., but by calcium and strontium only at a red heat. The density of metallic calcium prepared from the pure chlorid, was found to be 1·5778 as a mean of three experiments: the corresponding atomic volume is 158. The density of strontium as prepared from the pure chlorid and hammered out was found to be 2·5416 which corresponds to an atomic volume of 216. The author's former statement that calcium and strontium have a white color, is inaccurate. They both show under the polishing steel, as well as when reduced at a red heat and freshly cut, a beautiful gold yellow color. With respect to hardness, the metals belonging to this class may be arranged in the following order, each being scratched by those which succeed it. Na, K, Li, Pb, Ca, S_2. The order of their electrical relations is as follows, +K Na Li Ca Sr Mg −. From this it is evident that the electro-chemical position of strontium and calcium is different from what has been supposed. The dependance of the reducing power of the galvanic current upon its density, to which Bunsen first directed attention, has been very clearly shewn in the present investigation. The best arrangement is similar to that formerly described, and consists of a porcelain cru-

cible containing a porous cell, the fused chlorid standing much higher in the cell than in the crucible. The chlorine pole is a sheet iron cylinder placed outside the cell and the metal pole an iron wire passing through an earthen pipe stem within the cell, but not reaching the bottom. The heat is to be so regulated that the upper surface of the fused chlorid in the porous cell remains solid, so that the reduced metal can collect under it without touching the cell itself.—*Ann. der Chemie und Pharmacie*, xciv, 107, *April*, 1855. W. G.

II. Mineralogy and Geology.

1. *Hunt's Wilsonite a Scapolite*; extract from a letter from E. J. Chapman to Professor Dana.—Your surmises, as expressed in the last edition of the System of Mineralogy and in your letter to me of last month, have proved to be perfectly correct. The so-called Wilsonite is undoubtedly an altered variety of scapolite, notwithstanding my former measurements and the apparently oblique cleavage of the mineral or rock-mass. Before your letter reached me, I had arrived at the conclusion, in repeating my earlier investigations, that this oblique structure arose from rock-cleavage, so to say, or, as you express it, from the presence of joints. Being determined, if possible, to set the matter at rest, I took steps to procure some large specimens of the substance; and these I have now broken up and subjected to a very close examination. I find, that besides the more facile cleavages, there are others in the same general or vertical direction, but with exceedingly variable inclinations. All of these arise from a kind of composition parallel to the principal axis; and hence the variable measurements, and the error into which I was led in considering the substance to be triclinic. The specimens at that time at my disposal were not sufficient to enable me to detect this; but it is now quite evident that the cleavage planes belong entirely to the two forms *I* and *ii*. I enclose you some small specimens on which I had marked the positions of these forms, in order that you may test the justness of my observations.* The planes *ii* are the more readily obtainable of the two, but they scarcely admit, at least so it seems to me, of satisfactory measurement, the inclination oscillating around 90°. The *I* planes, however, are smooth and pearly, and give without the slightest doubt an angle of 90°. Besides which, the inclination of *I* on *ii* equals 135° exactly, in several of the fragments that I have measured; and hence, of course, *ii* : *ii* must equal 90°. Here, then, the dimetric crystallization of the substance is fully proved, and the cleavage directions shewn to be those of Scapolite.

In illustration of what I have stated respecting composition, I may mention that I have also obtained a cleavage plane lying between *ii* and *I*, exactly in the same zone, and giving with *ii* an angle of 126° or thereabouts—an impossible angle (as being less than 135°) unless caused by composition. You may remark the peculiarity in question in one of the specimens forwarded to you with this letter; and also an instance of cross composition—using the term in a wide sense—on the back of another specimen. In this manner I have found my former angles repeated more than once.—As you are aware, I have always

* The specimens confirm the statements of Prof. Chapman.—J. D. D.

looked upon the Wilsonite as an altered species of some kind, but I thought its affinities lay more with Anorthite (or Latrobite) than with Scapolite. It yet remains to be seen, however, if the Latrobite really belongs to the Anorthite. I cannot help suspecting its identity with Wilsonite; and, as you have shewn in the last edition of your Mineralogy, interchanges between Anorthite or some of its related feldspars and Scapolite, are by no means rare: witness, Walmstedt's Tunaberg mineral, porcelain spar, glaucolite, etc.

The altered condition of the mineral necessarily precludes us from drawing any safe deductions from its composition. A few remarks may, nevertheless, be ventured upon: but I should first premise that the amount of water, together with that of the intermixed carbonate, varies to the extent of 2 or 3 p. c. in different specimens. Taking Mr. Hunt's analysis, we obtain for the oxygen ratio between the Ä̇l and S̈i, 3 : 5 (or nearly so)—a ratio remarkable for the high value of the first term when compared with the generality of altered scapolites; especially when potash is present as a superadded base. Take, for instance, the Algerite, the yellow Scapolite from Bolton, etc. This may perhaps justify us in considering all the other ingredients as adventitious matters, replacing the original lime; and, in that case, we obtain, with very little latitude, for the Ċa, Ä̇l, S̈i, the oxygen ratio 1 : 2·5 : 4, as given below:

		Oxygen.	Ratio.
Silica,	47·50	24·66	4
Alumina,	31·17	14·57	2·5
Lime,	21·33	6·09	1
	100·00		

This ratio, as you have shewn in your Mineralogy, II, p. 202, is common to many scapolites.

I notice that Dufrenoy in his *Traité de Minéralogie*, vol. iii, has the following observations in reference to Amphodelite, with which Latrobite is now placed:—"The specimens of Amphodelite which I have had an opportunity to study, are amorphous. Their splintery-lamellar fracture places them very near Wernerite, with which, besides, they are almost identical in composition. I should therefore have united it to this species if M. Nordenskiöld, to whom we owe the description of the mineral, had not announced that it possesses two cleavages meeting at an angle of 94° 19′, from which he concludes that its primary is an oblique rhomboidal prism. * * I should remark, however, that the cleavage of this mineral is, by no means, good; and that the angle of 94° is not far removed from a right angle, so that the two may yet be brought together."

Toronto, Canada West, July 13, 1855.

2. *On Quartz*; by M. Descloizeaux.—M. Senarmont has made a report to the Academy of Sciences of considerable length, on an admirable memoir of M. Descloizeaux on Quartz, just published, from which we here cite.—After the discovery of the rotation of polarized light by quartz, in 1813, Herschel showed its connection with the left and right-handed characters of the crystallization—indicating a general law which has since taken a position of great importance, through the labors of MM. Pasteur and Marbach. Still its application to quartz was not without some anomalies; and although these irregularities, through

the labors of Brewster, Haidinger, and especially G. Rose, have been brought under a regular law, the subject has not been exhausted. The memoir of M. Descloizeaux affords means of judging on this point.

M. Descloizeaux has examined many crystals from various localities, noting that each locality has its characteristic forms. He has made out many new planes, increasing the number of observed rhombohedrons from 13 to 60, and of plagihedral planes from 23 to 66. In the rhombohedral series, he finds only 17 which coexist with the corresponding inverse rhombohedron; and in the plagihedral series, only 8, that have also the inverse. These facts, together with the different aspect and physical properties of the faces, are regarded as proving that the fundamental form is a rhombohedron instead of a hexagonal prism.

Besides the forms mentioned, M. Descloizeaux has made out many others which appear to require to be taken into account in the study of the geometric configuration of quartz, as they are regular and well defined, yet have quite high numbers in their crystallographic expressions. It is probable that these complex forms will increase as crystallography is more studied. The study of these facts, lying on the limits of possibility in crystallography, are of peculiar interest to the science. The preponderance of the regular forces is shown to be in a weakened state, permitting the effects of external perturbative influences. The same cause accounts for the frequent imperfection of the more complex forms and the variations of angle—imperfections seldom observed among the more common forms. M. Descloizeaux has given his measurements of these exceptional cases without disguising the discordances between calculation and measurements.

The pretentions, now common with regard to the absolute exactness of crystallographic observations, leading to the use of the method of least squares for estimating the probable errors, are a pure illusion. It is easy to show that artificial crystals are altered in angle by extraneous causes, foreign to the molecular forces; and it is impossible that natural crystals should have escaped like influences. When the number of minutes is somewhat uncertain, it is not reasonable to attach so much value to seconds: a number is reached; but when the anomalies are in the nature of the thing, it is only self-deception. The divergences have causes, which are to be discovered; and it is the great problem to ascertain the limits of variation, and not blindly to assume an illusory coincidence in accordance with the abstract law of Haüy.

The new faces determined by M. Descloizeaux have much increased the possible geometric elements of hemihedrism, with which the circular polarization appears to be connected. Twenty-four plagihedral faces are now known to occur either in the right-hand or left-handed spiral. M. Descloizeaux here inquires, what particular optical effect may be connected with each of these plagihedral forms. After numerous observations, he concludes that, with one exception, the facts thus far ascertained afford no absolute conclusion, and only suggest some probable inductions.

M. Descloizeaux next takes up the macles of quartz. He shows that they often obey only approximately the absolute law of twin-composition, the angles sometimes varying 2° from the theoretical value. This shows that the forms are not held inflexibly to a geometrical law; that the physical laws express only the limits of stable equilibrium

towards which the condition always tends, but from which it may diverge considerable from external causes, without destroying altogether the equilibrium.

The subject of composition by complete interpenetration and the soldering of crystals occupies much of M. Descloizeaux's memoir. The crystals are united irregularly, almost as if melted into one another, the union being in an exceedingly complex manner. These forms were well illustrated by Mr. Gustav Rose. But M. Descloizeaux has examined them by sections in various directions, and studied both their crystalline appearances and their optical characters, finding sometimes a mosaic interior.

The various appearances are shown in the engravings. The effects of polarized light were obtained by means of photography.

Plates cut from the extremities of the same crystal have rarely the same structure, and almost always show a more or less complicated arrangement of plates of contrary rotation.

The planes of composition are usually parallel to the faces of the pyramid, more rarely to the rhombic faces of Haüy; sometimes parallel to the faces of the hexagonal prism or some very acute rhombohedron. The arrangement of quartz of inverse rotation in layers parallel to certain faces of the crystal seems at first to accord with the law of increment. But how can we explain the direction of the different colored strata, when the parts of different rotation, while being still in regular layers, cross other plates often in opposite directions? when the layers have regular contours and divide the interior of the mass into compartments with a symmetry which cannot be mere chance? Photography has been of vast service to this subject by the fidelity with which it registers the facts. The plates are numerous and of the highest interest.—*L'Institut*, No. 1117.

3. *Note on Heulandite and Scapolite from Arendal*, by E. Zschau. —From the chemical composition of the varieties of these minerals which occur at Arendal may be noticed an interesting analogy. The following analyses (I, II) of *Scapolite* are by von Rath, and (III) of *Heulandite* by Sjögren.

	I.	II.
$\ddot{S}i$	45·05	46·82
$\ddot{A}l$	25·31	26·12
$\ddot{F}e$	2·02	1·39
$\dot{C}a$	17·30	17·23
$\dot{M}g$	0·30	0·26
$\dot{K}$	1·55	0·97
$\dot{N}a$	6·45	6·88
$\dot{H}$	1·24	0·33

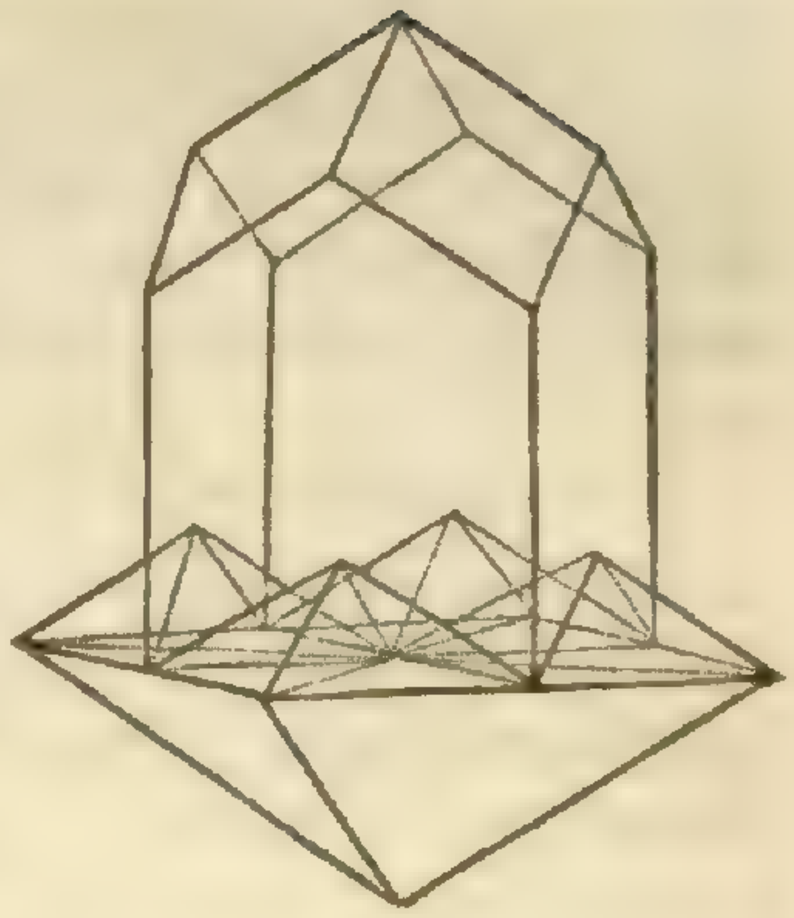
$(\dot{C}a, \dot{N}a)^3\,\ddot{S}i^2 + 2\ddot{A}l\,\ddot{S}i.$

	III.
$\ddot{S}i$	58·41
$\ddot{A}l$	16·56
$\dot{C}a$	7·89
$\dot{M}g$ Mn & alkalies	0·54
$\dot{H}$	16·53

$\dot{C}a\,\ddot{S}i + \ddot{A}l\,\ddot{S}i^3 + 6\dot{H}.$

The basic nature of Scapolite and the more neutral of Stilbite point out clearly that the excess of the bases in the former has been removed by atmospherical agencies, giving as products the more neutral and stable compound, Stilbite. The alkalies of the Scapolite have entirely disappeared, as well as a large portion of the earths.

This view is supported by the fact that the Heulandite of Arendal (Barboe Mine) always occurs either upon or near Scapolite.



4. *On a Twin Composition between Malacone and Xenotime, Apatite, or Monazite;* by ERNST ZSCHAU, (communicated by G. J. BRUSH.)—It is an interesting question whether crystals of different kinds can form twins with one another. The annexed figure of an actual crystal from Hitteröe, is an example of two species thus combined, both of the dimetric system, the prism being zircon (Malacone), and the octahedral part below, Xenotime; the two are homœomorphous, *O*: 1 being 138° 45′ in Xenotime, and but little less in Malacone (138° 15′).

Malacone also occurs at Hitteröe compounded with apatite and monazite. The *O* on 1 in apatite is 136° 47′.

5. *On Prosopite;* by GEO. J. BRUSH.—Prof. Scheerer* has shown that the so-called "Speckskein" pseudomorphs from Altenberg in Saxony, have the composition of Kaolin, and that the crystals although homœomorphs, are not pseudomorphs of Barytes as suggested by Breithaupt;—the original mineral being, according to him, a fluorid of aluminium and lime, with water, to which he has given the name Prosopite.

Subsequently Prof. Dana pointed out its homœomorphism with Datholite, and suggested that Prosopite might possibly be an altered Datholite.

Through the kindness of M. E. Zschau of Dresden, I was put in possession of a number of crystals of Prosopite, which, although presenting the same crystalline form, had such a diversity of physical characters, that I was led to give them a chemical examination, to ascertain if possible the true composition of the unaltered mineral.

Owing to the small quantity of the mineral at my command, the results obtained are for the most part incomplete.

The physical and chemical characters of the crystals were as follows: Color light violet and transparent to white and opaque. H. from 4 to so soft as to be easily scratched by the nail. Lustre brilliant to dull. The violet crystals had a hardness of 4, sp. gr. 3·177, a brilliant lustre, and a distinct octahedral cleavage; they contained no water and were composed of pure Ca Fl and a trace of organic matter. A quantitative analysis, made by decomposing with sulphuric acid, gave on 141·5 milligrammes of mineral employed, 242 milligrammes of sulphate of lime, equal to 71·44 milligrammes or 50·49 per. cent. of calcium. The result of the decomposition by sulphuric acid was completely soluble in water and the solution gave no reactions for any other base but lime. The crystals which had a less brilliant color and a diminished hardness contained more or less of a hydrous silicate of alumina according to the extent of alteration they had undergone, the results of complete alteration being pure kaolin as shown by Scheerer.

In all of the crystals that contained even traces of water, alumina was also found; and although silica was not in each case detected, there can be little doubt of its presence, as the change from fluor-spar into

* Poggendorff's Ann., xc, 315.

kaolin can be so distinctly followed. (Where fluor-spar predominated in the mineral silica might be easily overlooked as when decomposed by sulphuric acid, it would be evolved in the form of gas in combination with the fluorine. I confess I had neither apparatus nor sufficient quantity of the mineral to investigate this point as accurately as it deserved.)

This unexpected result made it of importance to determine with accuracy the form of the crystals. For this purpose all of the specimens in my possession were forwarded to Prof. Dana who has had the kindness to examine them and communicates herewith the result of his examination. The violet crystals were in some cases accompanied by massive fluor, in others imbedded in Hematite.

In connection with the above results, my attention was called to the fluor of other localities, particularly that of Zinnwald and Graupen. At Zinnwald the cubical crystals of fluor are sometimes altered into a substance resembling Speckstein; on analysis this so-called Speckstein proved to be a hydrous silicate of alumina and the partially altered crystals had the same composition as the Prosopite before examined.

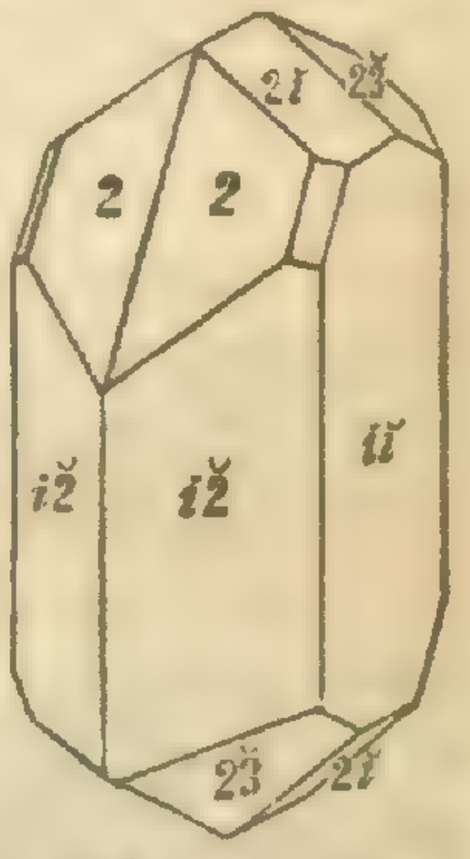
With the Cassiterite at Graupen in Bohemia is found fluor-spar and a compact white mineral which has heretofore been called Speckstein. This latter is a hydrous silicate of alumina and the same stages in the passage from fluor into this substance may be observed here as have been described with the Prosopite crystals. Specimens of fluor from Nieder, Pöbel, and other localities near Altenberg presented the same peculiarities.

From these facts it seems that fluor-spar is liable to alteration into kaolin, and that much of the so-called Speckstein is really a hydrous silicate of alumina.

I regret that it is not in my power to give a series of quantitative analyses showing the gradual passage of the fluor into kaolin, and I only offer this brief notice of my results in the hope that it may be the means of calling the attention of some chemist to the subject who may have an abundance of the mineral and a laboratory at his command.

Freiberg, July 16, 1855.

NOTE BY J. D. DANA.—I have examined the crystals of Prosopite sent me by Mr. Brush, and find the external characters and cleavage confirming fully his statements. The form presented, is well shown in the figure in my Mineralogy, 4th ed., p. 502, (here repeated) which is drawn from Scheerer's but in an altered position, and there is little variation either by distortion or otherwise. It is hemihedral trimetric, like Datholite. I have endeavored to reconcile the form with a distorted monometric crystal; but the persistence and regularity of the form in all the crystals, is against the idea of its being a distortion. Moreover, if a twin, composition must be parallel to *ĭĭ*; and this view is opposed by the inclination of *ĭĭ* to the planes around it, and still more by the cleavage, which cuts through the crystals without any reëntering angle or line of interruption. The cleavage affords in fact regular tetrahedrons and octahedrons with the same facility as fluor spar; and no one of the faces of cleavage in one crystal was



parallel to $i\breve{i}$, or to any other occurring plane. Moreover the cleavage planes had no symmetrical arrangement with reference to the axes of symmetry in the crystal.

Although, therefore, the violet crystals are monometric fluor spar in cleavage and other characters, I am not satisfied that the external form is monometric in origin; it may be pseudomorphous after some species not yet ascertained.

Several of the crystals have the edge between the two planes $i\breve{2}$ beveled by two planes I, which according to my measurements, (approximations only, as the faces do not reflect a distinct image and are often uneven,) incline to one another, 113° 30′. I obtained also $i\breve{2} : i\breve{2} = 74°\ 30'$, $I : i\breve{2} = 150°\ 45'$; $i\breve{i} : x$ (unlettered plane) 104° 30′; $2:2 = 131°$ in one crystal; $133° - 134°$ in another; (and the latter crystal afforded also $2\breve{i} : 2\breve{i} = 118°\ 15'$.) In one specimen there is a plane replacing the edge $i\breve{i} : 2\breve{i}$, giving for the inclination on $2\breve{i}$ approximately 149° 30′; it is probably the plane $6\breve{i}$, following the lettering in the figure. The plane $2\breve{3}$ is usually uneven; and there is sometimes another between it and $2\breve{i}$.

One of the Prosopite crystals is violet fluor at one extremity and white like a kaolin at the other.

6. *Note on Geology of country East of Cascade Monntains, Oregon,* (extract of a letter from Geo. Gibbs, Esq. dated Steilacoom, Washington Territory, May 7th, 1855).—Dr. George Suckley, Assistant Surgeon U. S. A., stationed at the Dalles of the Columbia, Oregon, has recently made an important discovery bearing upon the geology of the country east of the Cascade mountains, of which, although the details are as yet but imperfectly ascertained, we think a notice will prove interesting. It is the existence of extensive strata of fossiliferous limestone lying between beds of basalt. The limestone appears to be protected in places by a layer of river stones and gravel, and elsewhere to be metamorphic. The order of position descending is as follows. 1st. Sand and superficial soil 1 to 10 feet; 2nd, basaltic rock 3 to 10: 3d, cobblestone and pebbles; 4th, limestone 6 to 8 feet; 5th, cobble stones, &c.; 6th, basalt. Among the fossils are bones of mammalia, some of them gigantic. They are brittle and require great care in removing, and as yet no examinations have been made to ascertain their genera. These bones are also found near Fort Wala-wala, at the junction of the Snake and Columbia and are said by the Indians to be common there. No fossiliferous rock has heretofore, it is believed, been found in any place between the Blue Mountains and the Cascades and the rarity of organic remains, in that region has been the subject of remark. This discovery will throw much light on the character of the interior sea which once occupied that basin.

7. *Statistics of Coal*, including Mineral Bituminous Substances employed in Arts and Manufactures; with their Geographical, Geological and Commercial Distribution, and amount of Production and Consumption on the American Continent, with Incidental Statistics of the Iron Manufacture, by R. C. Taylor, F.G.S. London, etc., etc. 2nd edition. Revised and brought down to 1854, by S. S. Haldeman, Prof. Nat. Sci. and Agric. in Delaware College, &c. 640 pp. 8vo, with maps, plates, and numerous wood-cuts. Philadelphia, 1845: J. W. Moore.—The first edition of this work was issued in 1848. Since that time the able

author, Mr. R. C. Taylor has died, and this second edition has been brought up to the present time under the supervision and labors of Prof. Haldeman. The original work was a vast repository of knowledge on the subject of Coal, both scientific, practical, and commercial, a result of extensive personal knowledge of Coal mines and Coal, as well as of the literature of the subject. Moreover the subject of iron, particularly the iron trade—as it is so often a part of the industry of a coal region—came in incidentally and added greatly to the useful character of the work. In preparing the new edition, the work has been rendered more especially American, by enlarging the American part both with respect to the subjects of Coal and Iron, while the chapters treating specially of other parts of the world, have been omitted. These additions have been made with care and fullness, and the work is in itself a library on Coal, Coal Formations, all kinds of combustible materials in the earth, Coal mines and Mining, while it also contains much information on Iron, Iron furnaces, and the iron productions of different parts of the country and the world. New maps on the Coal formations of Iowa, Wisconsin, Illinois and Alabama have been added from the publications of Dr. Owen and Prof. Tuomey.

III. Botany.

1. *Sexual reproduction in the Lower Cryptogamia.*—The reprint of Henfrey's Report in the numbers of this Journal for January and March, 1853, followed in the May number (p. 455) by references to later investigations, brings down to a comparatively recent date the history of discovery in this important department of physiology. M. Thuret has continued his researches upon the fecundation of the *Algæ*, and, in Annales des Sciences Naturelles, vol. ii, No. 4 of the fourth series, announces that he has obtained direct proof of the impregnation of the spores by the antheridia in the common *Fucus vesiculosus* and other *diœcious* species, which on this account offer the best facilities for the investigation. The *octospores* having escaped from the conceptacles of the mother-plant, the 8 contained spores soon escape from the dissolving epispore, and from an extremely delicate membrane which Thuret finds within the latter. When they thus become free in the water (which they do within an hour or less after the octospores are shed from the conceptacles), the spores are round, olive-yellow bodies, entirely destitute of any integument; but after some hours a membrane of cellulose begins to form on their surface. In experiments upon these diœcious species the spores may be submitted to the action of the *antherozoides*, so abundantly shed from the antheridia of the male plants, which move free and spontaneously in the water with such vivacity, and which in this case come into direct contact with the naked spores;—or the latter may be kept separate from the former. In the first case *the spores are fecundated and germinate: in the second, they do not germinate at all.* On placing the spores in water containing some antheridia, the *antherozoides* were seen under the microscope to attach themselves in great numbers to the surface of a spore, and even to communicate to it a rapid rotatory movement, lasting for about half an hour,—which movement, as Thuret satisfied himself, was caused solely by the conjoint action of the

vibratile cilia of the attached *antherozoides*. The latter continue their usual movement, though with diminished vivacity, for some time after the spore comes to rest. The next day after the spores had been in contact with the *antherozoides* they were found to be clothed with a membrane of cellulose; and on the same or the following day, or sometimes later, the first partition appeared, dividing the spore into two cells; one of these began to form a slight protuberance at a point perpendicular to the partition, which soon elongated into a hyaline slender filament,—a sort of radicle, by which the germinating plantlet is attached,—while the cell at the opposite extremity enlarges and divides to produce the rudimentary frond. On the other hand, spores under the same circumstances, except that they were kept separate from the antheridia, remain unchanged for several days, and then are decomposed, or else they become covered with a cellulose coat and then make some abortive attempts at germination, but end by undergoing decomposition within ten or fifteen days. Having repeated these experiments many times upon all the diœcious species of the coast of Normandy, always with the same result, Thuret announces it as an ascertained fact, that the spores of *Fucaceæ* are incapable of germinating and of reproducing the species without the concurrence of the *antherozoides*. Moreover, no germinations could be produced when the spores of one species were mingled with the *antherozoides* of another of the same or of an allied genus,—showing the usual reluctance to hybridism;—except in the case of spores of *Fucus vesiculosus* mixed with the *antherozoides* of *F. serratus*, when germination readily took place; but the supposed hybrids, though well developed up to a certain stage, were not brought to perfection so that their character could be demonstrated. The striking resemblance of this whole process to that of fecundation in animals will not fail to be noticed. To the question whether the *antherozoides* penetrate into the interior of the naked spore, Thuret answers that nothing authorises him to think so, as he has never found any evidence of such penetration; while in certain species he has proof that the spores were fecundated by *antherozoides* which were arrested by a mucilaginous external coating produced by the dissolution of the epispore or otherwise.

In a second part of this memoir, published in Ann. Sci. Nat., ser. 4, vol. iii, No. 1, M. Thuret gives the result of his continued observations upon the fructification of the Florideous Algæ. In these, the antheridia, (which were first discovered by Ellis in 1757), have now been detected in the greater part of the species of all the principal groups; but the contained corpuscles analogous to the active *antherozoides* of the *Fucaceæ*, are not found to exhibit spontaneous movement, nor to be furnished with cilia or other locomotive organs. Probably antheridia exist in all these plants. Their general occurrence, and their position and structure seem to indicate their function. But while their mode of action is not known by observation, Thuret has ascertained that the spores may and often do germinate without any contact with the corpuscles.

Still later (as we learn from an abstract given by Henfrey, in Ann. and Mag. Nat. Hist. for May last), Dr. Pringsheim, of Berlin, has discovered *antherozoides* or *spermatozoids* in *Sphacelaria* and *Cladostephus*, resembling the corpuscles of *Florideæ* in form, but furnished with two

cilia like those of the *Fucaceæ*, and moving actively in the same manner. His observations upon the fecundation of *Fucus vesiculosus* confirm those of Thuret. Dr. Pringsheim has also found indications of bisexual reproduction in the green fresh-water *Algæ;* and in *Vaucheria* he thinks he has confirmed Vaucher's original idea that the 'horns' perform the function of anthers. These, according to his researches, are really antheridia, their contents being converted into minute vesicles, the $\frac{1}{180}$ of a line in diameter, furnished with two unequal cilia, by which, when set free by the bursting of the horn at the apex, they move rapidly in all directions, "great numbers (twenty, thirty or more) making their way into the opening of the spore-fruit, and coming into contact with the tough mucilaginous layer bounding the contents. After a time a distinct membrane appears all over the mass of contents of the spore-fruit, converting it into a free cell (the spore); and the author states that he several times saw a largeish, colorless corpuscle inside the mucilaginous coat of the contents; he believes this to have been a spermatozoid which had penetrated into the mass. The process as here described is therefore the impregnation of a mass of contents by a spermatozoid, and a subsequent formation of the cell-membrane of the spore." Pringsheim maintains here, and in a recent memoir on the structure of the cell in plants, that the primordial utricle of Mohl, the protoplasmic layer, does not cause the coat of cellulose or permanent cell-wall to be formed upon its surface, as Mohl maintains, but becomes actually transformed into the cellulose coat! We wait to learn how Mohl will regard this view. A. G.

2. *Trécul; Formations Secondaires dans les Cellules Végétales.*—An elaborate memoir, in Ann. Sci. Nat., ser. 4, vol. ii, illustrated by numerous figures, of which the principal conclusions are; that the vegetable cell can produce secondary formations or deposits on the outside as well as the inside of the primary membrane; some thickening in one way, some in the other, and some in both ways; that the spiricules, reticulations, rings, &c., are originated by the primary membrane which precedes them, and that they are *hollow;* that in the case of some vessels or cells composed of three zones, as in *Taxus*, the intermediate zone is the youngest; that, consequently, the secondary membranes are not due to sedimentary matters deposited by the liquids contained in the cells; and that the cavities of contiguous wood-cells communicate by real perforations of their parieties. A. G.

3. *Mohl on Chlorophyll.*—An article in the *Botanische Zeitung*, for February last, is reproduced by Henfrey, in an English translation, in Ann. and Mag. Nat. Hist. for May and June, 1855. With the thoroughness which characterises Prof. Mohl's researches, he first combats, apparently with complete success, Nageli's view of the utricular nature of chlorophyll-granules, showing that they have no investing membrane distinct from the contents, still less a cellulose coat comparable with the cell-membrane. He still believes that the grains of chlorophyll do not belong to the ternary series of products at all, but consist of a soft proteinous substance, related to albumen, in which in most cases one or more starch-grains are imbedded, and which owes its green hue to the presence of an extremely small quantity of green coloring matter, seemingly deposited only or principally in its outer layer. By demon-

strating the occurrence of chlorophyll in cells which contained no starch, or the growth of the green globules after the starch-grains have vanished, and in other cases the simultaneous increase of starch and chlorophyll grains in the same cells, Mohl has shown the groundlessness of Mulder's hypothesis, that chlorophyll is formed of starch altered by deoxydation, and that the evolution of oxygen by green foliage is merely the result of this supposed transformation of starch into green coloring matter and wax; and he maintains that, by appropriate evidence, he has abundantly demonstrated the principal mass of chlorophyll-grains to consist of a substance allied to protoplasm, which certainly cannot originate from a metamorphosis of the constituents of starch. A. G.

4. *The Seeds of Magnolia.*—In a paper read before the Linnæan Society in November last (of which an abstract is given in *Ann. and Mag. Nat. Hist.* for May last), Mr. Miers is led to question the view taken in the *Genera of N. Am. Plants Illustrated*, viz, that the fleshy integument of the seed of Magnolia is a proper seed-coat, arising from the external coat of the ovule; and, indeed, he insists that it is an *arillus*, or in other words an accessory covering, not found in the ovule but formed at a later period. A careful re-investigation of the ovules and seeds in a fresh state enables us positively to state that the latter supposition is incorrect; that no accessory covering is formed over the ovule or seed; that the outer coat of the former, which is thick and fleshy from the first, and has the vessels of the rhaphe imbedded in it, thickening still more as the seed grows, becomes the red fleshy envelope, while a series of new cells formed on its inner face, and elongating transversely as they multiply by division, gives rise to the crustaceous integument; the inner coat of the ovule meanwhile remaining thin, and forming a delicate membrane more or less closely adherent to the albumen. Mr. Miers' view, therefore, is correct only as respects the innermost covering of the seed. A. G.

5. *Bertoloni: Miscellanea Botanicá*, fasc. 13 & 14. Bologna, 1853.—These fasciculi contain each an academical lecture, and the characters of a few supposed new species of plants. In the 13th, as in several former fasciculi, the materials are unfortunately furnished by a set of the specimens collected many years ago in Alabama by Dr. Gates, and distributed to subscribers. We enumerate the seven plants here named, described, and figured, to show what Signor Bertoloni makes of them.

(1.) His *Nicotiana humilis*, a new species with opposite leaves! is ***Dipteracanthus ciliosus***, Nees.

(2.) *Persea longipeda*, of which the author may truly say, "Non reperio inter Perseas hactenus evulgatas," since it is evidently *Chrysobalanus oblongifolius*.

(3.) *Trichostemma linariæfolium*, is luckily of the right family and genus: it is *T. lineare*, Nutt.

(4.) *Dasystoma tubulosa* is *Macranthera fuschioides*, Torr.

(5.) *Hypericum punctulosum* is *H. cistifolium*, Lam. (*H. opacum*, Torr. & Gr.)

(6.) *Ascyrum Plumieri* is *A. Crux-Andreæ*, Lin.

(7.) *Empetrum aciculare* is *Ceratiola ericoides*, Michx. A. G.

6. *Prof. Braun*; "*On the oblique direction of the ligneous fibre, and the twist of the trunks of trees occasioned thereby*;" read before the Academy of Sciences at Berlin, and published in its Proceedings.—This twist of the wood of many trees is a phenomenon well known to wood-cutters, shingle-makers, carpenters, and others, but almost entirely neglected by botanists. The distinguished geologist, the late Leopold von Buch, appears to have first directed the attention of scientific men to it; and De Candolle, in his *Organographie* (1827) was the first botanist who spoke of it. Little has since been done to substantiate or elucidate the phenomenon. In the pamphlet before us Prof. Braun gives the result of a great many observations made by himself in Germany, by his brother in France and Spain, and by the writer of this notice in the Mississippi Valley.

Most trees show this obliquity of the woody fibre more or less. In certain species the twist is almost uniformly in the same direction: in others both directions occur with about equal frequency; while in not a few no twist is distinctly observable. Sometimes the same direction prevails in the majority of the species of a genus, or even of a whole family: in other cases opposite directions occur in the same genus or family; and it is curious to remark that in some instances nearly allied species of Europe and America twist in opposite directions. In a few instances the fibre of a young tree is twisted in one direction; that of the old tree in the opposite direction.

In speaking of the direction, it is necessary to come to an understanding, first of all, as to what we mean by right or left, a distinction attended with more difficulty than would appear possible. Prof. Braun follows De Candolle and others in viewing the twist or coil objectively; imagining himself in the centre of the coil. Thus viewed, the bean-vine turns to the *left*, the hop-vine to the *right*, &c. Linnæus and others, however, have adopted the opposite, or subjective view, and regard the bean and other leguminous plants as turning to the right, as they appear to an observer standing before the coil.

The twist of the fibre may be discerned in splitting the wood, or in its cracks when the bark is stripped off, or in the course of the fissures made by lightning. Very often the bark itself, at the angles or superficial lines of the trunk, indicate the direction of the wood within very distinctly. We make a few extracts from 167 species observed.

No manifest twist has been observed in species of *Fagus*, *Juglans*, and *Carya*, either in Europe or America, nor in *Ulmus*, *Ailanthus*, *Fraxinus*, *Acer dasycarpum*, *Gleditschia* or *Robinia*, though the latter exhibits a very slight twist to the left. The woody fibre twists to the right in *Pinus Strobus*, *Ostrya Virginica*, the Chestnut of Europe, the European and American *Salices*, *Populus pyramidalis*, *Cornus Florida*, *Liriodendron* (in Indiana and Illinois, though in cultivated specimens the twist was found to be the other way; but more observations are required), the Peach, Plum, and Cherry trees, and in the European *Cercis Siliquastrum*, the only Leguminous tree known to twist to the right. The twist to the left hand is the more common: it occurs in most *Coniferæ*, especially in *Juniperus Virginiana*, *Taxodium distichum*, *Pinus sylvestris* (of which young trees twist, however, in the opposite direction), *Picea excelsa*, &c., *Betula* and *Alnus*, *Ostrya vul-*

garis and *Castanea Americana* (both in opposite direction to the nearly allied species of the Old World), *Quercus Robur*, *Populus angulata*, *Catalpa*, *Æsculus Hippocastanum*, the Pear tree, and more than any other the Pomegranate, also most Leguminous trees.

Most American Oaks, the Sassafras, *Acer nigrum*, the Apple-tree, &c., twist about as often to the right as to the left.

The cause of the apparent twisting is not easily ascertained. It is not occasioned by an actual twisting of the whole stem, but belongs to the growth of the successive annual layers. Prof. Braun connects it with the growth of the wood-cells, of which the ends, at their formation are horizontal or nearly horizontal, become wedge-shaped as they elongate; and if these wedges assume the same direction in the whole circumference of the stem, as they are apt to do, the wood-cells would assume a certain obliquity; so that this twist of the wood is connected with the intimate nature or disposition of the cells themselves. But this is not sufficient to explain the higher grades of the obliquity, which sometimes reaches an angle of 45 degrees. It would here be desirable to ascertain whether the cambium cells divide in this oblique direction and high angle.* Any one who may take an interest in these investigations is requested to institute observations and make memoranda, noting the number of trees observed, as well as the direction and angle of the twist observed, if any. G. E.

7. *The Wellingtonia of Lindley.*—In a communication to the Botanical Society of France in June, 1854,† M. Decaisne has confirmed the view which we had suggested, in this Journal and elsewhere, that the gigantic Coniferous tree to which Dr. Lindley gave the name of *Wellingtonia gigantea* is nothing more than a second species of *Red-wood* or *Sequoia*, which must therefore bear the name *S. gigantea*. Decaisne justly observes that two forms of foliage occur in a more or less marked manner in most *Coniferæ*, not excepting the *Sequoia sempervirens*, which on the same branch often presents both imbricated and distichous leaves;—that in the structure of the fruit and seeds the common Red-wood and the so-called *Wellingtonia* are identical, the only difference being that the cones of the latter are larger;—that the structure is not like that of *Sciadopitys*;—and finally, that the wood of the two trees is similar, both abounding with the red coloring matter, soluble in water, from which the Red-wood takes its name. M. Decaisne, however, appears not to have seen the male flowers, which, as we formerly remarked, are alone wanting to confirm the perfect generic identity of *Wellingtonia* with *Sequoia*.

From M. Boursier de la Rivière, the French consul in California, M. Decaisne has likewise received specimens of a new California Conifer,

* Our common Tupelo, or Pepperidge, or *Nyssa* would be a good subject for the investigation, since the obliquity of its wood is generally very considerable, and the layers of a certain number of years incline in the opposite direction from those of the preceding layers. A. G.

† *Bulletin de la Société Botanique de France, fondée le 23 Avril*, 1854. Vol. 1, No. 2, p. 72.—This is published in monthly numbers, each of 44 to 60 pages, 8vo. We have received the first volume and two fascicles of the second of this publication, and find it full of interesting matter, showing the great activity and ability with which this Society has begun its work. M. Brongniart was the first President, and was succeeded by M. Decaisne for the current year.

which he refers to *Chamæcyparis*, and names *C. Boursieri*, distinguished by its short leaves, very closely imbricated, oval, acuminate leaves, furnished with a gland about the middle.

Decaisne distinguishes from our *Taxodium* the Mexican tree, under the name of *Taxodium Montezumæ*. A. G.

8. *Does Sea-water kill Seeds?*—A question which has an important bearing upon the actual or possible dispersion of many species over the large geographical area which they are found to occupy, and therefore upon the problem whether the same organic being was created at one point, or at several or many widely separated points, on the face of the globe. It is commonly believed and stated that seeds—those of maritime plants excepted—will not germinate after exposure to salt water: and so general is the belief, that no one, so far as we know, has made the experiment until now, when the distinguished naturalist, Mr. Darwin, has shown that seeds of various kinds will germinate promptly after prolonged immersion in sea water. The account of his simple but well-devised experiments is given in the London *Gardeners' Chronicle* for May 26th, 1855. We copy the principal part of it.

"As I had not the least notion when I began, whether or not the seeds would be all killed by a single week's immersion, I at first took only a few, selecting them almost by chance from the different great natural families; but I am now trying a set chosen on philosophical principles, by the kindness of Dr. Hooker.

"The sea-water has been made artificially with salt procured from Mr. Bolton, 146, Holborn Bars, which has been tested by better chemists than men, namely, by numerous sea animals and algæ having lived in it for more than a year. The seeds were placed in separate bottles, holding from two to four ounces each, out of doors in the shade: the mean temperature has during the period been about 44°, rising during one week to a mean of nearly 48°. Most of the seeds swelled in the water, and some of them slightly colored it, and each kind gave to it its own peculiar and strong odor. The water in which the Cabbage and Radish seeds were placed became putrid, and smelt offensively in a quite extraordinary degree; and it is surprising that any seeds, as was the case with the Radish, could have resisted so contaminating an influence; as the water became putrid before I had thought of this contingency, it was not, and has never been, renewed. I also placed seeds in a quart bottle in a tank filled with snow and water, to ascertain whether the seeds kept at the temperature of 32° would better resist the salt water; this water, like that in the small bottles, to my surprise, became turbid and smelt rather offensively. In the following list I have no reason to suppose, except in the cases where so stated, that the seeds have endured their full time.

"(1) Seeds of common Cress (Lepidium sativum) have germinated well after 42 days' immersion; they give out a surprising quantity of slime so as to cohere in a mass. (2) Radishes have germinated less well after the same period. (3) Cabbage seed: after 14 days' immersion only one seed out of many came up; I think this is rather strange considering that the cabbage is a sea-side plant; in the ice-cold salt water, however, several have come up after 30 days' immersion. (4) Lettuce seed has grown well after 42 days; (5) of Onion seed only a

few have germinated after the same period; (6) Carrot and (7) Celery seed well after the 42 days; (8) Borago officinalis, (9) Capsicum, (10) Cucurbita ovifera, have germinated well after 28 days' immersion; the two latter, rather tender kinds, were also tried in the ice-cold water, and have germinated after 30 days' immersion. (11) Savory, or Satureja, has grown somewhat less well after 28 days. (12) Linum usitatissimum: only one seed out of a mass of seeds (which gave out much slime) came up after the 28 days, and the same thing happened after 14 days; and only three seeds came up after the first seven days' immersion, yet the seed was very good. (13) Rhubarb, (14) Beet, (15) Orach, or Atriplex, (16) Oats, (17) Barley, (18) Phalaris Canariensis, have all germinated excellently after 28 days; likewise these six latter after 30 days in the ice-cold water. (19) Beans, and (20) Furze or Ulex: of these a few survived with difficulty 14 days; the Beans were all killed by 30 days in the ice-cold water. (21) Peas germinated after seven days, but were all dead after 14 days' immersion out of doors, and likewise after 30 days in the ice-cold water. (22) Trifolium incarnatum is the only plant of which every seed has been killed by seven day's immersion; nor did it withstand 30 days in the ice-cold salt water. (23) Kidney Beans have been tried only in the latter water, and all were dead after the 30 days.

"As out of these 23 kinds of seed, selected almost at hap-hazard, the five Leguminosæ alone have as yet been killed (with the exception of the Cabbage seed, and these have survived in the ice-cold water), one is tempted to infer that the seeds of this family must generally withstand salt water much worse than the seeds of the other great natural families; yet, from remarks in botanical works, I had expected that these would have survived longest. It has been really curious to observe how uniform, even to a day, the germination has been in almost every kind of seed, when taken week after week out of the salt water, and likewise when compared with the same seeds not salted—all of course having been grown under the same circumstances, namely, in glasses on my chimney-piece, so that the seeds from the day of being planted have been always under my eye. The germination of the Rhubarb and Celery alone has been in a marked degree altered, having been accelerated. With respect to Convolvulus tricolor, not included in the above list, I may mention that many of the seeds germinated and came out of their husks, while still in the salt water, after six or seven days' immersion.

"To return to the subject of transportal, I may state that in 'Johnston's Physical Atlas' the rates of 10 distinct currents in the Atlantic (excluding drift currents) are given, and the average of them is 33 nautical miles per diem; hence in 42 days, which length of immersion seven out of the eight kinds of seed as yet tested have already stood, a seed might be readily carried between 1300 and 1400 miles.

"I will conclude this too lengthy communication by observing that all the 40 to 50 seeds which I have as yet tried sink in sea-water: this seems at first a fatal obstacle to the dissemination of plants by sea currents; but it may be doubted whether most seeds (with the exception of the winged kinds), when once shed, are so likely to get washed into the sea as are whole or nearly whole plants with their fruit, by

being carried down rivers during floods, by water-spouts, whirlwinds, slips of river-cliffs, &c., continued during the long lapse of geologically modern ages. It should be borne in mind how beautifully pods, capsules, &c., and even the fully expanded heads of the Compositæ, close when wetted, as if for the very purpose of carrying the seed safe to land. When landed high up by the tides and waves, and perhaps driven a little inland by the first inshore gale, the pods, &c., will dry, and opening will shed their seed; and these will then be ready for all the many means of disposal by which Nature sows her broad fields, and which have excited the admiration of every observer. But when the seed is sown in its new home, then, as I believe, comes the ordeal; will the old occupants in the great struggle for life allow the new and solitary immigrant room and sustenance?"

It would be well to submit to this experiment the seeds of a considerable number of those very species which naturally occur in two or more widely separated areas. And also to take the seeds of indigenous individuals, rather than of those of long-cultivated plants, which last certainly possess augmented vegetative power, though perhaps they may not offer increased resistance to the action of salt water.

We have just learned from Mr. Darwin that some of these seeds have germinated after 82 and 85 days' immersion in sea-water, namely, those of Radishes, Beet, Atriplex, Capsicum, Oats, Cucurbita, Rhubarb, Lettuce, Carrot, Celery, and Onions. A. G.

9. *Ravenel; Fungi Caroliniani Exsiccati.* Fasciculus III. John Russell, Charleston, S. C. 1855.—This fasciculus appears to be an improvement on the preceding ones, especially in its almost entire exemption from typographical errors, by which the others are so much defaced. The index has a much neater appearance. The specimens are generally good, and in sufficient quantity for illustrating the species. Foreign purchasers will be pleased to find so large a proportion (this century contains over sixty) of species peculiarly American. Many of these were never before published. Several fine species have been furnished from beyond the limits of Carolina; as the pretty *Aregma speciosum*, Fr., by Dr. Sartwell of New York; *Bulgaria rufa*, Schw., by Dr. Michener of Pennsylvania; a new *Cladosporium* by Mr. Beaumont of Alabama; the fine *Stereum pergameneum*, Berk. & Curt., and *Xylaria Œdipus*, Mont., &c., by Hon. T. M. Peters of the same State. This latter gentleman has been particularly successful in the discovery of new species of cryptogamic plants, thus supplying a new instance of what can be done for the advancement of science in the leisure intervals of professional life. Mr. Ravenel has himself given several fine forms of *Corticium* and *Sphæriceæ*; a new genus RAVENELIA, founded on the *Sphæria epiphylla*, Schwein., of which Mr. Berkeley has sent us a second species from India, and two very pretty epiphytes growing upon Toxicodendron, viz: *Uredo Toxicodendri*, Berk. & Rav., and *Pileolaria brevipes*, B. & R. This last has a very suspicious affinity to *Uromyces*, and unless some character more important than the length of its pedicel should be found to distinguish *Pileolaria*, this new species will present a good reason for uniting the two genera.

We have not made any critical examination of the species in this Fasciculus, but we should judge that there would be little need of future correction. *Polyporus dealbatus*, n. sp., does not seem to be the plant upon which that name was originally imposed, but agrees with what we have in our collection as *P. luteus*, Nees. *Sphæria elongata* is a species of *Valsa*, an error too extreme to be accounted for but on the supposition of extreme carelessness. But as these two errors exist in the volume through Mr. Ravenel's too great reliance upon ourself, we will share their responsibility with him. M. A. C.

IV. Astronomy.

1. *Comet*, 1855, II, (Astron. Journal, No. 85).—A new comet was discovered at Florence on the third of June last, by Dr. Donati. It was also seen the next day at Göttingen by Dr. Klinkerfues, and at Paris by Mr. Dien. Dr. Donati was unable to detect nucleus or tail, and estimated it as fainter than the nebula in *Hercules*.

From the observations at Berlin, June 5, 6, 7, Mr. Bruhns has computed the following elements:

Perihelion passage, 1855, May 29, 23896,	Berlin M. T.
Long. perihelion, - - -	240° 15′ 18.″4
" asc. node, - - -	260 52 43. 1
Inclination, - - -	22 58 27. 1
Log. q, - - - -	9.745678
Motion retrograde,	

2. *Report on the Shooting Stars of August 9th and 10th*, 1855; by Edward C. Herrick.—The following observations made at New Haven, Connecticut, show that the meteoric display of August has occurred this year with the usual characteristics.

The evening of Thursday, August 9, was clear and beautiful. Between $10^h\ 25^m$ P. M. of the 9th and $2^h\ 30^m$ A. M. of the 10th, *three hundred and eighty-five* different shooting stars were noted by three observers, viz, Prof. Alex. C. Twining, Mr. Christopher D. Seropyan and myself.

Many of these meteors were brilliant, and about four-fifths of all were conformable to the usual apparent radiant, or point of divergence, near the cluster in the *sword-handle* of Perseus. The following table shows the number of meteors seen per hour in the different parts of the sky, and also the number of those which were conformable to the radiant:

$10^h\ 25^m$ to 11^h P. M.,	E.	18,	of which 12 conformable.
	S. W.	14	7
	N. N. W.	17	12
		— 49	— 31
11^h to midnight,	E.	37	27
	S. W.	26	22
	N. N. W.	16	13
		— 79	— 62

Midn. to 1h A. M. (10th),	E.	42	37 *c.*
	S. W.	30	27
	N. N. W.	26	22
		— 98	— 86
1h to 2h	E.	53	44
	S. W.	26	24
	N. N. W.	31	24
		— 110	— 92
2h to 2h 30m	E.	22	16
	S. W.	14	10
	N. N. W.	13	9
		— 49	— 35

During the night of Friday, the 10th, the sky was somewhat embarrassed by clouds during the whole period, about one-sixth part being on an average obscured. Observers: Prof. Alexander C. Twining, Mr. Francis Bradley, Mr. Ch. D. Seropyan. Different meteors seen, *two hundred and ninety*, as follows:

10h 30m to 11h P. M.,	N. N. E.	11,	of which 7 conformable.
	W.	19	14
	S. E.	15	14
		— 45	— 35
11h to midn.,	N. N. E.	40	34
	W.	34	27
	S. E.	35	26
		— 109	— 87
Midn. to 1h A. M. (11th),	N. N. E.	53	47
	W.	32	26
	S. E.	51	47
		— 136	— 120

The night of the 11th was clear. Mr. Bradley, watching alone, looking chiefly towards the N. and E., observed *thirty-seven* meteors in the hour from 10 to 11 P. M., about half of which were conformable. Professor Twining watching alone, and having the sword-handle of Perseus in the centre of his field of view, observed from 1h 55m to 3h A. M. of the 12th, *fifty-eight* meteors, of which forty-five were conformable. The visible courses were shorter than on the two previous nights, and fewer trains were left.

The night of the 12th was also clear. Prof. Twining, watching alone, from 1h 30m to 2h 45m A. M. of 13th, observed *thirty-six* meteors, of which twenty appeared to diverge from the constellation Perseus, and eight from some point near the zenith.

Tuesday night (14th) was nearly clear. Between 1h 10m and 2h 10m A. M. (15th) observations were made by myself with Charles C. Herrick, and *thirty-three* different shooting stars were seen, viz, 16 in N. N. E. and 17 in the west. Of these most were so remote that the place of the radiant could not be well determined. Of a few the divergent point

was about a degree vertically above the cluster in the sword-handle of Perseus, and many of the others were generally conformable to that vicinity.

On the same morning Prof. Twining observed *sixteen* meteors, from $3^h\ 8^m$ to $3^h\ 40^m$ A. M.

On the night of the 9th, the place of the radiant, or centre of the area in which the visible paths of the meteors traced back would intersect, was in my judgment about R. A. 35°, North decl. 56°.

The position of this point among the stars seems to have undergone no very certain change since 1837 and 1838, before which it was not so well ascertained.

From about midnight onward we saw on the first night of observation, (9th), a faint light hardly a degree high, along the northern horizon, which we supposed might be the Aurora Borealis. At 1 A. M. this appearance was not discernible in the North, but the Zodiacal Light was then becoming apparent in the N. N. E., although its outlines could hardly be determined. By 2 A. M. the Zodiacal Light was quite conspicuous, yet ill defined, appearing in position and extent as at this period in former years.

A faint illumination of the Northern horizon was also observed on the night of the 10th, but on account of clouds its character could not be certainly determined.

New Haven, August, 1855.

V. Miscellaneous Intelligence.

1. *The Smithsonian Institution.*—*Note.*—In our article upon this Institution, in the July number of the Journal, on p. 10, we find that we have unintentionally done injustice to the views and consistent course of action of the Hon. J. A. Pearce, of the U. S. Senate ;—having been misled by placing a reliance on the correctness of certain statements in "Mr. Meacham's Report," (p. 258,) which, it appears, we ought not to have done.

It certainly involves no unfavorable implication that a person should change his mind on further experience, and under a change of circumstances. And, assuming the full correctness of Mr. Meacham's account, our object was to show 1st, how naturally and properly a Senator might have advocated in Congress "the library plan," in preference to any other then proposed, and afterwards, as a Regent, cordially sustain the plan of operations actually adopted : and 2nd, that the doing so clearly proved that he did not understand the law as it passed to prescribe "the library plan," to the exclusion of the other active operations which have been so successfully undertaken.

We find, however, that Mr. Pearce has steadily supported the plan which was finally adopted from the time of its adoption to the present, —a plan which combines the library idea with the scheme of scientific inquiry and investigation.

Mr. Meacham's account, in giving (p. 257) first a summary of Mr. Choate's arguments for the amendments introduced by him into the bill reported by Mr. Tappan, and immediately following this by the statement that "Mr. Pearce also advocated the views of Mr. Choate ;" and

by quotations as if from a lengthened speech,—conveys, and seems intended to convey the impression that Mr. Pearce addressed the Senate in support of Mr. Choate's distinctive propositions. Whereas, a reference to the *Congressional Globe* of January 13, 1845, shows that Mr. Pearce's few remarks were made before Mr. Choate's propositions were brought in, and his speech "at great length upon the subject" delivered; and that they were made upon a special question, namely, upon striking out a provision in Mr. Tappan's bill requiring an exclusively scientific library, viz: "that the books to be purchased for said Institution shall consist of works on science and the arts, especially such as relate to the ordinary business of life, and to the various mechanical and other improvements and discoveries which may be made." Moreover, as between Mr. Tappan's general plan, then before the Senate, which provided for the appointment of a staff of salaried professors, and for an institution modeled on the plan of the Imperial *Jardin des Plantes*, menageries and all, to be supported on the small income of Smithson's bequest, and a plan contemplating mainly a library, it is not surprising that a person of such sound practical judgment should have preferred the latter.

Perhaps it is worth noticing, finally, that the several extracts from Mr. Pearce's remarks on this occasion as given in Mr. Meacham's Report (inclusive of three lines, not put under quotation marks) actually comprised his *whole speech*, and indeed more words than the full report in the *Congressional Globe* contains.

2. *Introduction of Bombyx Cynthia into Europe.*—The introduction of this Silkworm into Europe, which is apparently attributed to Prof. Milne Edwards, in the January number last, took place as follows, as we learn from a Periodical published at Calcutta:

"An Italian gentleman, Signor Bergonzi, being desirous of introducing the Palma Christi silkworm into Piedmont, applied through a common friend to Mr. Piddington for a supply of the eggs, which were obtained through Mr. Payter of Bogorah and sent to Professor Baruffi of Turin, but after repeated and varied trials it was found impossible to make the eggs reach Italy in a living state. Sir William Reid having been appointed Governor of Malta, it was thought that the chances of success would be much increased if the insect could be first propagated at Malta, where the Palma Christi is abundant, and where it would also be a great acquisition to the poor but industrious population of that island, in which expensive attempts to introduce the mulberry have wholly failed from the poverty of the soil. That Sir William Reid heartily lent his coöperation we need not say; and after a series of trials and failures too long to relate here, and at the end of *two years* of patient endeavors, a few of the moths, the progenitors of which had been bred in Calcutta by Mr. Piddington, and were actually spinning their cocoons on board the steamer on her passage down the river, in boxes contrived to afford them air and every convenience, laid their eggs, probably in Egypt, and the next generation of worms were born and reared in the Palace of the Knights of St. John of Malta!

"From Malta, eggs have already been sent to Piedmont and to Rome, and we believe to Egypt. Sir William Reid contemplates distributing them, with the aid of the Peninsular and Oriental Company,

who have handsomely given their best assistance 'o this useful enterprise, on all the shores and islands of the Mediterranean, and he has been furnished, (as well as the Royal Agricultural Society of Turin, which has taken an earnest interest in the project,) with complete directions for the breeding and manufacture of the silk by the gentlemen we have named above; and these will be no doubt translated and circulated so as to enable the poor peasantry of those countries to find considerable resource from a plant which abounds every where about their cottages and fields."

3. *American Association for the Advancement of Science.*—The ninth meeting of the American Association was held at Providence, during the week commencing with August 15th, Dr. JOHN TORREY being the President of the meeting. The next meeting was appointed to be held in August, 1855, at Albany, N. Y., and Prof. JAMES HALL of Albany was elected President for that meeting and the ensuing year.

The contributions to the meeting, in addition to the Address of the retiring President, J. D. DANA, and a Report on the Progress of Organic Chemistry, by Dr. WOLCOTT GIBBS, were as follows:

(1.) *Astronomy and Mathematics.*

On Solutions by Geometrical Construction. By THOMAS HILL, of Waltham, Mass.

New Tables for determining the values of those co-efficients of the Perturbative Functions of Planetary Motion depending upon the ratios of the mean distances. By J. D. RUNKLE, Naut. Alm. Cambridge.

New Tables for converting longitudes and latitudes in A. R's and Decl. By J. D. RUNKLE.

Researches in Analytic Morphology—Transformation of curves. By BENJAMIN PEIRCE, Cambridge.

Solution of Adam's Prize Problem for 1857. By BENJAMIN PEIRCE, Cambridge.

The determination of Longitudes by Occultations of the Pleiades. By BENJAMIN PEIRCE, Cambridge.

Partial Multipliers of Differential Equations. By BENJAMIN PEIRCE, Cambridge.

The Catenary on the vertical right cone. By BENJAMIN PEIRCE, Cambridge.

The motion of a Heavy Body on the circumference of a circle, which rotates uniformly about a vertical axis. By BENJAMIN PEIRCE, Cambridge.

Resistance to the motion of the Pendulum. By BENJAMIN PEIRCE, Cambridge.

Contributions to the Atmospherology of the Sun. By Dr. C. H. F. PETERS, Cambridge.

On the temperature of the Planets, and on some of the conclusions resulting from this temperature. By Prof. ELIAS LOOMIS.

On the mean distance from the Sun, Inclination of Orbit, and Equatorial characters of the Asteroid Planets. By Prof. STEPHEN ALEXANDER.

On the physical phenomena presented during the Solar Eclipse of May 26, 1854. By Prof. STEPHEN ALEXANDER.

On the colored projections from the edge of the sun, as observed during solar eclipses. By Prof. J. HENRY.

Some additions to the new mode of astronomical observations in Right Ascension and in Declination. By Prof. O. M. MITCHEL.

On the Zodiacal Light. By Rev. GEO JONES, Chaplain U. S. N.

(2.) *Physics and Chemistry.*

Notice of earthquake waves on the Western Coast of the United States, on the 23d and 28th of December, 1854. By Prof. A. D. BACHE.

Approximate Cotidal lines of the Pacific Coast of the United States, from observations in the United States Coast Survey. By Prof. A. D. BACHE.

Notice of Tidal Observations made on the coast of the United States in the Gulf of Mexico, with type curves at the several stations, and their decomposition into the curves of diurnal and semi-diurnal tides. By Prof. A. D. BACHE.

Discussion of the secular variation in the Magnetic Declination on the Atlantic and Gulf Coast of the United States, from observations in the 17th, 18th, and 19th Centuries. By CHAS. A. SCHOTT, United States Coast Survey.

A method of producing parabolic fluid mirrors for Reflecting Telescopes. By GEORGE R. PERKINS.

On our sense of the Vertical and Horizontal, and our perception of Distances. By Lieut. E. B. HUNT, U. S. A.

On Binocular Vision. By Prof. W. B. ROGERS.

On the Rotascope or Gyroscope, and the composition of Rotations. By Prof. WM. B. ROGERS.

Improvements in the Electric Telegraph, whereby two or more terminal stations can make simultaneous use of the same wire. By MOSES G. FARMER, of Boston.

Description of a new apparatus for separating Gold and other precious metals from foreign substances. By EDWARD N. KENT.

On a method of analyzing the Sulphate, Arsenate, and Molybdate of Lead. By Prof. J. LAWRENCE SMITH.

(3.) *Geology and Mineralogy.*

On the stratigraphical position of the coal-bearing rocks below the upper Red Shale and Carboniferous Limestone of the Middle and Southern States. By Prof. WILLIAM B. ROGERS.

On the Configuration of the soil of New England. By Prof. A. GUYOT.

On the occurrence of iron ores in the Azoic System. By J. D. WHITNEY, Esq.

On some new localities of minerals. By SANDERSON SMITH.

On some crystallized furnace products. By SANDERSON SMITH.

Contributions to our knowledge of the Geology of Nebraska and the Mauvaises Terres. By Prof. JAMES HALL.

Notes upon the genus Graptolithus. By Prof. JAMES HALL.

On the Development of the Septa in the genus Baculites, from the extreme young to the adult state. By Prof. JAMES HALL.

On the cutting and polishing of Granite and hard materials by drifting sand in the Colorado Desert. By W. P. BLAKE.

Notice of remarkable specimens of crystallized and arborescent gold, from California. By W. P. BLAKE.

On the deposits of fossil microscopic organisms at Monterey, California, with specimens; observations on the characters and probable Geological age of the Sandstone formation of San Francisco, California; on the Cinnabar Mine of New Almaden, California. By W. P BLAKE.

Description of Yellow Creek Coal Basin. By R. P. STEVENS.

Sulphuret of Iron formed in Coal since the Drift Era. By R. P. STEVENS.

Conclusions from a series of facts respecting the erosions of the Earth's surface, especially by rivers. By Prof. EDWARD HITCHCOCK.

Marks of Ancient Glaciers in New England. By Prof. EDWARD HITCHCOCK.

Notes on the Geology of Western India. By Rev. E. BURGESS.

Remarks on the Geological formation of Table Mountain, Cape of Good Hope. By Rev. E. BURGESS.

On the occurrence of Proboscidean remains in Wisconsin. By EDWARD DANIELS.

On the character of the Lead deposits of the Upper Mississippi. By EDWARD DANIELS.

On the occurrence of silicious grits as veinstones in the Lead mines of Wisconsin. By EDWARD DANIELS.

Some observations on the northern outcrop of the Illinois Coal formation. By EDWARD DANIELS.

On minerals of the Wheatley Lead Mine. By Prof. J. LAWRENCE SMITH.

(4.) *Zoology and Botany.*

On gradation among Polypi. By LOUIS AGASSIZ.

On the System of Zoology. By LOUIS AGASSIZ.

Notes on the nature of the coverings of the seeds of Magnolia, and on the dicœcious character of species of Plantago. By Prof. A. GRAY.

Motions effected by plants result from the contractions of cells. By Prof. A. GRAY.

Notes upon the grove of mammoth trees of Calaveras Co., California; and on a peculiarity of the wood of the red wood (genus Sequoia) of California. By WM. P. BLAKE.

(5.) *Meteorology.*

On the Storm of Oct. 7, 1854, near the coast of Japan, and the conformity of its progression with other cyclones. By WM. C. REDFIELD.

On the storm which was experienced throughout the United States about the 20th of December, 1836. By Prof. ELIAS LOOMIS.

On the Winds. By Capt. CHARLES WILKES, U. S. N.

(6.) *Miscellaneous.*

On the frozen wells of Owego, Tioga Co., N. Y. By Prof. JOHN BROCKLESBY.

Notes on the Wilmington Gunpowder Explosion. By Prof. DENISON OLMSTED, of New Haven.

On the effect of mingling radiating substances with combustible materials. By Prof. JOSEPH HENRY.

Account of Experiments on the alleged spontaneous separation of Alchohol and Water, made at the Smithsonian Institution. By Prof. HENRY.

Mode of testing building materials, and an account of the marble used in the extension of the Capitol. By Prof. HENRY.

On an Index of Papers on subjects of Mathematical and Physical Science. By Lieut. E. B. HUNT, U. S. A.

On the use of salt marsh sods for facing the steep slopes of parapets, terraces, &c. By Lieut. E. B. HUNT, U. S. A.

Contributions to the Physiology of Sight. By Dr. T. C. HILGARD.

Laws of reproduction considered, with special reference to the marriage of near blood relations. By Rev. CHARLES BROOKS.

4. *The Climate of San Francisco, for the year* 1854; by H. GIBBONS, M.D.—The year began with very fine weather. On the 5th, was a severe norther which damaged the shipping in the harbor. A few cold mornings followed, and on the 12th the rains set in. From that date to the 24th, rain fell on 9 days, to the depth of $4\frac{1}{4}$ inches. The rains were cold, and several times accompanied with hail, and snow covered the distant mountains. The coldest weather on my record was at this time. On the 19th, 20th and 21st, the thermometer stood at 31, 25 and 31. At noon on the 20th, it rose no higher than 37. The mud was frozen solid so as to bear the heaviest wagons. Of course, the oldest inhabitant had never seen the like. In December, 1850, it was nearly as cold, the mercury then falling to 27. The month wound up with a few very warm days. The mean temperature at sunrise was 42·26, at 9 A. M. 45·71, at noon 54·23, at 10 P. M. 45·26, being the coldest month on my book—*i. e.* since the winter of 1849–50. The greatest heat was 69, and the extreme of cold 25. The prevailing winds were N, NE, and NW.

It should be mentioned, as a rare phenomenon, that hail fell on the morning of the 15th, so as to cover the ground and it lay for an hour. In the winter of '49–50 the ground was covered with hail or snow in like manner.

February was rather warm. The mean at sunrise was 47·93, at 9 A. M. 50·86, at noon 59·21, and at 10 P. M. 49·07. The extreme of heat was 69, of cold 38. Rain fell on no less than 13 days, and in the quantity of 8·41 inches—the greatest quantity in any month on my record, excepting Dec. 1852, when there was nearly 12 inches. This is the more remarkable as February is usually a dry month. On the 12th hail fell in a heavy shower so copiously that it could be gathered by the bucket full where it collected from the roofs. The prevailing winds were from W, S, NW and N, in the order named as to frequency. Our high wind occurred from SSE.

March was of moderate temperature. The mean at sunrise was 47·23, at 9 A. M. 52·06, at noon 60·97, at 10 P. M. 49·45. The extreme of heat was 72, of cold 38. Rain fell on 10 days 3·17 inches, a moderate supply for March. Most of the rain was during a cold storm on the 13th, 14th and 15th, the wind blowing moderately part of the time from Northeast, which is a rare direction for a rain-wind. The Westerly winds increased in frequency, as usual in this month. Those from S, NW and N, divided among them one-half the month. There were no high winds.

The warmest April on my book was that of 1854. The mean at sunrise was 51·10, 9 A. M. 59·83, noon, 68·43, 10 P. M. 52·90. The extreme of heat was 83, of cold 45. Rain on 6 days, 3·31 inches, nearly two inches of which fell on the 28th,—the last rain of the season. The sea-breeze came nearly every day, though with moderate force. On 10 days the winds were from other quarters than West. During this month the hills and fields assumed the gorgeous array of flowers which marks a California landscape in the spring.

May was a very unpleasant month, cold and windy, often cloudy and threatening rain. On one day only there was rain, and then but two-hundredths of an inch, in the form of mist. The mean temperature at sunrise was 48·95, at 9 A. M. 59·00, at noon 64·61, at 10 P. M. 50·68 —being three degrees below April. The mercury rose no higher than 73, and the lowest extreme was 43. There were light frosts on several mornings, and vegetation advanced tardily. Potatoes of the season's growth appeared in market on the 1st. A hail storm occurred at Sacramento on the 6th. The winds were westerly on 25 days. On 8 days they were high.

June, also, was a cold month, rather below April in temperature. Mean at sunrise 50·10, 9 A. M. 61·83, noon, 66·80, 10 P. M. 51·50. The warmest day was 74, and the coldest morning 47. There was an unusual tendency to rain, and several times a few large drops deigned to violate the law of the season. On the 17th it rained moderately for two hours, four-tenths of an inch collecting in the gauge. On the 13th was a heavy storm of rain and hail in Utah. On 23 days the wind was West, and on four NW. It was high on eight days.

July was rather above the average temperature. The means were, at sunrise 51·87, 9 A. M. 63·94, noon 70·65, 10 A. M. 54·16. In the three years preceding, the mercury had not reached 80 in July, but in this year it was at or above 80 on four days, and on one day as high as 87, which is near the extreme heat of our climate. The lowest extreme was 46. The first week was beautifully clear, but afterwards there was scarcely a morning or evening without cloud and mist. The wind was constantly West, and on six days it was high.

August was a trifle below the average temperature. Mean at sunrise 52·42, 9 A. M. 62·39, noon 68·29, 10 P. M. 53·81. There were two days above 80, the highest being 85. The minimum temperature was 50. Almost every afternoon was windy, and though the wind was high on one day only, yet the weather was about as unpleasant as our summer climate can afford. The mornings were generally cloudy and the evenings misty. A light shower of rain fell on the 27th. At Los Angeles and San Diego it rained heavily on the 20th and 21st, and on the

Trinity river there was a thunder storm on the 26th, with heavy rain and snow on the mountain peaks.

September, commonly the warmest month in the year, was nearly as cold as August. Mean at sunrise 53·30, 9 A. M. 61·43, noon 67·73, 10 P. M. 54·40. There were two warm days, on one of which the mercury rose to 87. The greatest depression was 46. Cloudy mornings and misty evenings prevailed, and the sea breeze blew with great constancy and with more force than usual in September. This month seldom passes without rain, but on the present occasion the only rain was a trifling shower on the 15th. There was a heavy rain at Los Angeles about the same time.

The weather of October was generally agreeable. Mean temperature at sunrise 53·32, 9 A. M. 60·97, noon 68·13, 10 P. M. 55·42. There were three days above 80, the warmest being 83. The minimum temperature was 46. It was the warmest month of the year except July. The winds were light, and distributed to W, NW, N, and S, the first predominating. The most extraordinary feature of the month was its frequent rains. Rain fell on no less than 10 days, quantity 2·12 inches. The first rain was on the 4th. At Marysville the ground was covered with hail on the 23d. At the close of the month the hills around the city began to look green, and the wise men predicted a very rainy winter.

The climate of November was fine. Mean at sunrise 50·67, 9 A. M. 55·97, noon 65·13, 10 P. M. 53 00. The extremes were 72 and 47. The mornings ranged from 47 to 55, and the noondays from 58 to 72. The winds were from W, NW and N, and gentle as zephyrs. The sky was most uninterruptedly clear. A single rain fell, amounting to four-tenths of an inch, and the wise men reversed their prediction and promised a very dry winter instead of a wet one.

December furnished a continuation of the fine weather of November, with a decline of temperature corresponding to the season. Mean at sunrise 47·03, 9 A. M. 51·32, noon 60·65, 10 P. M. 49·39. There were a number of slight frosts, and ice formed in favorable situations, though the minimum temperture was 38. The warmest day was 71. The most gentle breezes prevailed from North, Northeast and Northwest. There were 15 days entirely clear. A trifling rain, ·08 inches, fell on the 3d, and no more until the 31st, when a rain storm set in which was quickly handed over to the new year, leaving three-tenths of an inch to December.

The mean temperature of the whole year sums up as follows: Sunrise 49·68; 9 A. M., 57·11; noon, 64·57; 10 P. M., 51·76. The mean of the sunrise and noon observations gives as the figure for the year, 57·13. The temperature for 1851, deduced in the same way, was 56·57; 1852, 56·53; and 1853, 58·51. The year 1853 appears to have been unusually warm. Taking 1854 as a fair representation of the climate of San Francisco, it follows that our climate is two or three degrees warmer than that of the corresponding latitude on the Atlantic coast, though it exhibits neither the extremes of heat or cold incident to the latter.

The extreme of heat in 1854 was 87. There were only twelve days in the year at or above 80, of which one was in April, 4 in July, 2 in

August, 2 in September, and 3 in October. In 1851 there were 9 days at or above 80; in 1852, 13; and in 1853, 11.

The extreme of cold was 25. There were three days in the year when the mercury fell to the freezing point, all in January. In 1851 the thermometer fell to the freezing point on one day only. In 1852, 35 was the lowest depression; and in 1853, it did not sink below 40.

The warmest month in the year was July, then October, then September, then August, then April, and June stands the sixth in order, and only two degrees above November. In neither of the three years preceding, was July the warmest month. In 1851 the warmest months occurred in the following order: August, October, September, June, July, April. In 1852, September, July, August, June, October, November. In 1853, October, September, June, May, July, August. To the daily visitations of the cold ocean wind in the summer is owing this great variation from the order of the months as to comparative temperature in other climates.

January was the coldest month, then February, then December and next March. In other years December sometimes takes the precedence of January. February, which in the Atlantic States is often the coldest month in the year, is not so here.

Rain fell on 54 days in the year, 22·12 inches in depth. This is our average supply, though only half the quantity that falls in the Atlantic States. In 1853, the quantity was 19·03 inches; in 1852, 25·60 inches; and in 1851, only 15·12 inches. The old inhabitants tell of occasional seasons when scarcely any rain has fallen, and when the cattle have perished from want. Such very dry seasons are said to recur at intervals of 8 or 10 years.

The greatest amount of rain was in February—next comes January, next April, then March. This differs from the ordinary arrangement. Taking the last four years into view, December gives the most rain, and March comes next, while the intervening months are comparatively dry. In fact we have the early rains, beginning in November and continuing through December into the early part of January, and the later rains, beginning in March, and continuing at times through April.

Lightning is seen at San Francisco on an average three or four times a year, and thunder is less frequent. On the 15th January, flashes of lightning were observed in the evening during a cold rain storm from South; and on the 22d February, under similar circumstances lightning was again noticed. But no thunder was audible in either case; nor was there any further exhibition of atmospheric electricity during the year. There was thunder three times in 1851, five times in 1852, and twice in 1853.

No exhibition of Auroral light was observed in the year. Since my residence here, from Aug. 1850, I have seen the Aurora Borealis only on two occasions, once in January and once in February, in the year 1852.

Earthquake shocks were distinctly felt on the mornings of the 9th of January and 21st of October.

5. *Iron ore of Dodge and Washington Cos., Wisconsin.*—Dr. PERCIVAL, in a report on this ore, states that it occurs in a bed 7 to 15 feet thick, lying between two strata of limestone, " that below obviously cor-

responding in its physical characters and fossils to the upper shell bed of the Blue limestone of the mineral district, and that above to the upper magnesian limestone. The iron is argillaceous and mainly is lenticular ore (called also seed or shot ore). The iron ore layer has a slaty structure. It is usually overlaid by a thin layer of a very dark blueish and hard compact ore, breaking with a conchoidal fracture. The bed is one of great value, and of inexhaustible extent, and occurs in a densely timbered country.—*Rep. by Dr. J. G. Percival, dated Milwaukee*, 1855.

6. *Clouds.*—Photography has been used successfully in Paris in taking views of clouds. They have been obtained by Bertsch in hardly a quarter of a second in a style which leaves nothing to be desired. They are adapted to resolve, as M. Pouillet states, all the important questions relating to their form, distribution and height. M. Pouillet measures the height by means of two photographic apparatuses, placed at a distance from one another.—*L'Institut*, No. 1118, June, 1855.

7. *Pottsville Scientific Association.*—The first Bulletin of this Society, for January and February of the current year has been issued, in a pamphlet of 14 pp., 8vo. It contains a translation by Mr. I. O. Meredith of a paper by Mr. Desor on the Falls of Niagara.

8. Obituary.—*Notice of the late John Graem Ellery.*—By the death of John G. Ellery, science has lost one of its most energetic and indefatigable workers, who could ill be spared from his favorite field of investigation.

He died on the 2nd of June, 1855, at Gold Hill, Rowan Co., N. C., of one of those virulent attacks incident to the climate. He had been engaged there for some months in a Geological Survey, having direct reference to the value and most economical mode of working the gold, silver, and copper ore of that region, and was just about to return to New York, when he was prostrated by sickness.

Mr. Ellery was educated in part at Hamilton College, N. Y., and afterwards graduated at Amherst College, Mass. Having chosen the department of geology and mineralogy as the great study of his life, he turned his attention after leaving college, to the study of chemistry, as the groundwork of his preparation, for the special object of his pursuit. His studies were completed at the Royal Academy of Mines, in Freiberg, Saxony, where in an unusually short time, he not only familiarized himself with the principles of mineralogy and geology, but also mastered the practical departments of Mining and Metallurgy. He was well versed in the department of analytical chemistry, and under Plattner became especially proficient in the use of the blowpipe. It was his ambition to fit himself for developing the mineral resources of his own country, and to this end his efforts were unwearied. His devotion to science as well as his true manliness of character, won for him there as well as here, the highest respect and confidence of his teachers. His career since his return from Europe, though brief, had been full of promise. He had already accumulated a fund of information with regard to the mineral resources of North Carolina that would soon have been made available to the public, had he not fallen thus prematurely. All who knew him merely as a man of science, feel most deeply the loss to themselves and to the world: but besides these, a wide circle of

friends, who appreciated not only his scientific qualifications, but his many excellencies of character, his integrity of purpose, his native kindness of heart, his self-sacrificing spirit, feel more deeply than words can express the affliction that has come upon them.

New York, June 9, 1855. A. K. E.

Mr. Thomas Weaver.—The veteran Geologist, Mr. Thomas Weaver, died at his residence in Stafford place, Pimlico, on the 2nd inst., in the eighty-second year of his age. The contemporary of Humboldt and Leopold von Buch, he acquired, in company with these illustrious men, his rudiments of mineralogy and geology under the tuition of Werner, at Freiberg, having been entered on the books of that celebrated Mining Academy in 1790. Among his numerous memoirs published in the "Transactions of the Geological Society," those upon "The East and South of Ireland" are perhaps the most praise-worthy, for the service they rendered to the development of the mineral structure of that country. Mr. Weaver was, until within these few years, a frequent contributor to the Philosophical Magazine and other scientific periodicals. Amid the geologists of this country, no one was more distinguished for a kind and friendly disposition, which endeared him to all his associates.—*Athen.*, *July* 7, 1855.

Andrew Crosse.—Mr. Andrew Crosse, the enthusiastic and somewhat eccentric amateur of science—whose experiments in electricity and his fancied creation of insects some years ago made a stir among the credulous and incredulous—died the other day at his residence near Bridgewater, having passed the threescore years and ten allotted to man.—*Athen.*, *July* 21, 1855.

9. *Report of Exploration of a Route for the Pacific Railroad*, near the 47th and 49th Parallels, from St. Paul to Puget Sound; by I. I. Stevens, Governor of Washington Territory. H. Doc., 129.—Governor Stevens's Report extends to 600 pages 8vo, and embodies details of his own observations relating to regions hitherto little explored, and also various accompanying reports occupying 500 pages of the volume.

In the Report of Mr. George Gibbs, it is stated that Mount Baker, near Puget's Sound is said to have been in slight eruption during the winter of 1853–'54, throwing out light clouds of smoke. The last eruption preceding was in 1843 when a slight shock of an earthquake was felt at Port Langley. He observes also that St. Helens was smoking; adding,

"This [St. Helens] and Mount Baker are the only volcanoes at present active in the chain. Its last considerable eruption was in 1842, when it covered the country as far as Vancouver and the Dalles with ashes, and presented a luminous appearance after the smoke had cleared off. The Indians report that there were once three mountains that smoked always, Mount Hood and Mount Adams being the others. Respecting Mount Hood and Mount Adams, they have a characteristic tale to the effect that they were man and wife; that they finally quarrelled and threw fire at one another, and that St. Helens was the victor; since when Mount Hood has been afraid, while St. Helens having a stout

heart, still burns. In some versions this story is connected with the slide which formed the Cascades of the Columbia, and by damming up the water inundated the forest, the remains of which are now visible along its margin. The date of this event Lewis and Clark fixed at about thirty years before their arrival. It is very probable that it may have been due to an earthquake, as earthquakes, though not frequent, are known upon the coast. The Indians have no tradition of an eruption of lava; they have only seen smoke and ashes come out of the mountain. They add that a bad smell came from it, and that the fish in the streams died. Around the foot of St. Helens, they say, the ashes are so deep and soft that horses cannot travel."

Mr. Gibbs gives the following account of the Coal of Bellingham Bay:

"I visited the coal-beds on the D'Wamish and at Bellingham Bay, but I had no time for making more than a very superficial examination. That on the outlet of D'Wamish lake is situated immediately upon the water, a few rods below Tobin & Co.'s mills, and about a mile from the lake. The outcrop is exposed to the eastward or river side, and dips to the water at an angle of about 15°, being broken off towards the north by a fault. The total depth of the bed, including shale, is 13 feet, with 6½ feet of workable coal, divided by a thin seam of fire-clay. The drift had been carried in only about 75 feet. The coal appears to be of good quality, and to burn well with a strong flame, leaving no slag. It is considerably laminated, and has a tolerably bright fracture. The indications are said to continue westward for a considerable distance. Another bed has been discovered lower down, on the main D'Wamish, at Stevenson's claim. It is intended to transport this coal in scows to the town of Seattle, about sixteen miles distant by water.

"Two beds had been opened at Bellingham bay, and the coal was visible in the immediate bank for a long distance. The beds are from 2 to 16 feet thick, and dip to the north at angles ranging from 19 to 37 degrees. The overlying rock is a light-colored yellowish sandstone, containing pebbles and concretions of clay. No associated fossils were seen. The shales are very thin as far as noticed, and limestones entirely wanting. Some of the outcrops of coal appear to be at the edge of faults, but the thickness of the formation itself was not examined. The seam which had been most worked—that known as the Ma-moosie mine—was altogether eight feet through, but divided by three feet of clay and slate, leaving only an equal amount of workable coal. A drift had been carried in about 175 feet, the quality improving somewhat. The superior seams at this place did not appear to be worth working. About 150 tons only had been got out, which was mostly on board a vessel bound to San Francisco.

"Another bed, a little to the north of this, belonging to Captain Fauntleroy and others, presented much better indications. Its thickness is sixteen feet four inches, and the coal brighter and freer from impurities than the other. A small quantity got out here sold in San Francisco for $23 per ton. A bed of nearly equal depth is said also to have been since found in the southern part of the Bay. Other beds have

been found on Samish bay, and Mr. H. A. Goldsborough saw it upon the Stoluckwamish in workable seams, but not accessible to water transportation. The coal at Bellingham bay, must be lightered on board of vessels, the water being shallow to a considerable distance from the shore.

"The whole of this formation has been considered by geologists as tertiary, and the coal as not belonging to the true coal. Be this as it may, its value for economical purposes is unquestionable. Even that on the Cowlitz and Skookum, though inferior to the product of the D'Wamish and Bellingham bay mines, was abandoned only from its not being accessible to tide-water."

10. *Report of Exploration of a Route for the Pacific Railroad*, near the 38th and 39th Parallels of Latitude from the mouth of the Kansas to Sevier River in the Great Basin; by Lieut. E. G. BECKWITH, Third Artillery. Also Report, by the same, of Explorations on the line of the 41st Parallel of North Latitude, 1854.—Both reports contain much important information concerning the geography and productions of the routes traversed.

11. *Report of a Reconnoissance and Survey in California*, in connection with Explorations for a Practicable Railway route from the Mississippi river to the Pacific Ocean, in 1853; by Lieut. R. S. WILLIAMSON, U. S. A., Corps of Topographical Engineers. 80 pp., 8vo.—Lieut. Williamson's Report includes the Geological Report of W. P. Blake, noticed in the preceding volume of this Journal, p. 433.

12. *Report of Exploration of a Route for the Pacific Railroad*, near the 32d parallel of latitude from the Red River to the Rio Grande; by Brevet Captain JOHN POPE, Corps of Topographical Engineers. H. Doc., 129.—Besides the general account of the country traversed, the volume contains a short Geological Report by JULES MARCOU, and a Catalogue of the Plants, by Dr. JOHN TORREY, with descriptions of a few new species. The evidence of Jurassic rocks on the Rocky mountains which Mr. Marcou presents is wholly unsatisfactory. Cretaceous beds were found as far as the "upper cross Timbers" in longitude 97° 30′ and latitude 33° 40′.

13. *Report of Explorations for a Railway Route* near the 35th Parallel of Latitude from the Mississippi river to the Pacific Ocean; by Lieut. A. W. WHIPPLE, Corps of Topographical Engineers. H. Doc., 129.—The general description of the route and country is followed by a Geological chapter by Jules Marcou; another on Economical Zoology, by C. B. R. KENNERLY, and on the Soil, Productions and Special forest trees, by J. M. BIGELOW, Botanist, besides tables of heights and distances, and another of Meteorological observations.

14. *Report of Explorations for that portion of a Railway Route* near the 32nd Parallel of latitude lying between Dona Ana on the Rio Grande and Pimas Villages on the Gila; by Lieut. JNO. G. PARKE, U. S. A., Corps Topog. Engineers. H. Doc., 129.—Lieut. Parke visited Tuczon and saw the famous meteorites there which have been described in this Journal by Prof. Shepard and Dr. J. Lawrence Smith. The Report contains information on the region passed over with a table of calculated Barometrical heights.

15. *An Examination, by direction of the Hon. Jefferson Davis, Secretary of War, on the Reports of Explorations for Railroad Routes from the Mississippi River to the Pacific*, made under the Orders of the War Department in 1853–'54, and of the Explorations made previous to that time, which have a bearing upon the subject; by Capt. A. A. HUMPHREYS and Lieut. G. K. WARREN, Corps Topog. Eng., Washington, 1855. H. Doc., 129.—A valuable review of the Facts collected in different Explorations of the Rocky mountains, with reference to the Pacific Railroad route.—Also a Report of the SECRETARY OF WAR on the several Pacific Railroad Explorations.

16. *An Introduction to Practical Astronomy, with a collection of Astronomical Tables;* by Prof. ELIAS LOOMIS, LL.D., &c.—Every cultivator of practical astronomy must have felt the want of some treatise embracing within moderate compass the requisite formulæ and tables, together with descriptions of the most approved instruments and methods of observation. The only work in English to which he could refer—Pearson's—besides being bulky and very expensive, is out of date.

This want it has been the aim of Prof. Loomis to supply. Amateur observers, surveyors, scientific travelers, and students of practical astronomy generally, will welcome the present work as one especially adapted to their necessities.

The chapters on instruments are clear, comprehensive and practical. Demonstrations accompany the mathematical formulæ, so that the student has, at every step, the means of satisfying himself of the accuracy of the methods adopted. This characteristic of the work will give it especial value as a college text-book. The accurate computation of eclipses and occultations, and the determination of longitudes from observations of these phenomena, have received particular attention. In no other English treatise, so far as we are aware, are Bessel's formulæ so well developed and applied.

A marked feature of the work is the catalogue of 1500 stars, embracing all above the sixth magnitude, and giving their positions with all the accuracy of the most approved standard catalogues. This feature will be especially valued by those who are unable to purchase the expensive British Association and Twelve Year Catalogues, from which this has chiefly been taken, or who desire, in a more portable form, a catalogue of such stars as come within the range of ordinary instruments. The constants for computing the apparent places are also given. Great care seems to have been taken to render not only this catalogue, but the other tables, and the work generally, as reliable as possible. The author's known habits of discrimination and scrupulous accuracy are a sufficient guaranty that in this particular the book will be found unusually free from fault.

The work being intended only as an introduction, and containing less than 500 pages, no one will expect it to treat of the higher and more intricate branches of practical astronomy, such as the computation of orbits, perturbations, etc. But to the classes for whom it was prepared it can safely be recommended, as having, for their purposes, no superior in the English language, if, indeed, in any other.

17. *The Relations of Chemistry to Agriculture, and the Agricultural Experiments of Mr. J. B. Lawes*; by JUSTUS VON LIEBIG. Translated by SAMUEL W. JOHNSON, at the author's request. 88 pp., 12mo. Albany, N. Y., 1855.—This little work is a translation of a recent agricultural treatise by Prof. Liebig, by one who has the full confidence of its author, both in his knowledge of agricultural chemistry and of the language in which it is written. Mr. Johnson has resided at Munich for the past year and a half, where, while pursuing his studies, he has had constant intercourse with the eminent Professor of that University.

18. *A Treatise on Pneumatics*, being the Physics of Gases, including vapors: containing a full description of the different Air Pumps, and the experiments which may be performed with them; also the different Barometers, Pressure Gauges, Hygrometers, and other Meteorological Instruments, explaining the Principles on which they act, and the modes of using them. Illustrated by numerous wood engravings, by MARTIN H. BOYE, M.D., A.M., Prof. Nat. Phil. & Chem., &c. 116 pp. 8vo, with several tables. Philadelphia, 1855. E. C. & I. Biddle.—This treatise, by one of the best chemists of Philadelphia, is written with precision and clearness, and will be found a valuable and convenient work. It is well illustrated with cuts, and contains tables for Barometric and Hygrometric calculations, including tables for the tensions of vapor of water, calculated from those of Regnault, being reduced to English measures and the Fahrenheit scale.

19. *Fossil Footmarks in the Red Sandstone of Pottsville, Pennsylvania;* by ISAAC LEA, LL.D., Vice President of the Acad. Nat. Sci., Philadelphia, &c. 16 pp. super-royal folio, with one double lithographic plate. —The fossil footmarks found by Dr. Lea in the vicinity of Pottsville, and described and figured by him in the Proceedings of the American Philosophical Society for June, 1849, and thence cited in this Journal, are here figured anew of natural size, on a plate of magnificent dimensions as well as beauty of finish. The slab itself measures three feet in length by two in breadth, and contains six tracks in a double row, the tracks duplicated by the hind foot falling into the impression of the forefoot, but a little more in advance. As these tracks have already been described in this Journal, we will not enter into details at this time. Dr. Lea argues that the rock is Devonian. It is No. XI. of Prof. Rogers, a formation generally regarded as above the Catskill group, and constituting the lowest portion of the beds of the carboniferous age, probably synchronous, with some part of the carboniferous limestone.

The typography of this work is beautiful beyond any thing hitherto published in this country, well comporting with the majestic size of the page.

20. *Fossils of South Carolina;* by M. TUOMEY and F. S. HOLMES. Charleston, S. C., 1855. John Russell.—The appearance of the first number of this beautiful work was announced in a former volume of this Journal. Nos. 2, 3, 4, 5, 6, sustain the same elegant style of typography and plates. Each species described is figured, and the lithographs are remarkably fine in drawing and engraving. These numbers contain many species of Molluscs, Echinoderms and Corals. We com-

mend the work for its beauty as well as its science, and would urge all to subscribe for it, who wish it completed.

21. *The Natural History of Man:* comprising Inquiries into the Modifying Influence of Physical and Moral Agencies on the different Tribes of the Human Family; by JAMES COWLES PRICHARD, M.D., F.R.S., M.R.I.A., &c.: 4th edition, edited and enlarged by EDWIN NORRIS, of the Royal Asiatic Society of Great Britain and Ireland. Illustrated with 62 colored plates, engraved on steel, and 100 engravings on wood. In two volumes, of 720 pages, 8vo. 1855. London, H. Baillière.—The great value and interest of Prichard's "Natural History of Man" has long been acknowledged. It is the only work in our language that takes up this subject in so extended and thorough a manner, and the whole is presented in a style at the same time popular and judicious. The present edition is issued on fine paper, with a large number of the 60 steel plates of faces colored. Many additions have been made to the volumes by the editor, Mr. E. Norris, without any mutilations of the original work, or any modifications beyond what was required by the new information added.

22. *Quekett's Practical Treatise on the use of the Microscope*, 3d edition, forming vol. vi, of the "Library of Illustrated Standard Scientific Works," 556 pp., 8vo, with 10 plates and many woodcuts. London, 1855. H. Baillière.—This edition of Quekett's Treatise on the Microscope, a work already familiar to our readers, has just been issued in superior style by H. Baillière.

23. *Human Longevity and the amount of life upon the Globe;* by P. FLOURENS, perpetual Sec'y. to the Academy of Sciences, Paris, &c., &c. Second edition, translated from the French by CHARLES MARTEL. 198 pp., 12mo, 1855. London: H. Baillière.—We have derived from this book both amusement and instruction. The author extols the happy calmness of old age, and argues from physiology, hygiene and analogy that the natural period of man's life is one hundred years, and that if his life is cut short before that term he is killed—he does not *die* before the full century. Taking into view the fact that the perfect union of the bones with the epiphyses takes place in man at 20 years, and regarding this anatomical sign as a proof of complete development—as the close of the period of youth—and further assuming the period of growth as one-fifth of the whole duration of life, he arrives at one hundred years as the full term of life for man. He observes, by way of analogy, that—

In the Camel this union takes place at	8	years;	period of life 40 years.
Horse, - -	5	"	" 25
Ox, - - - -	4	"	" 20
Lion, - - - -	4	"	" 20
Dog, - - - -	2	"	" 10
Cat, - - - -	18	mon.	" 9 to 10
Rabbit, - - -	12	"	" 8
Guinea Pig, - -	7	"	" 6–7

The book will repay a careful perusal.

24. *Annals of the Astronomical Observatory of Harvard College*, vol. i, Part II, 1852–1853. Printed from funds resulting from the will of

Josiah Quincey, Jr., who died in April, 1775, leaving a name inseparably connected with the history of the American Revolution. 316 pp., 8vo. Cambridge, 1855.—This volume contains the results of Astronomical Observations made at the Harvard Observatory under the direction of WILLIAM CRANCH BOND, A.M., Fellow of the American Academy of Arts and Sciences, &c., GEORGE P. BOND, A.M., 1st Assistant, CHARLES W. TUTTLE, A.M., 2nd Assistant. It includes a Zone Catalogue of 5500 Stars, between the equator and 0° 20′ N. Declination, observed during the years 1852–53. The tables contain the right ascension and declination of all stars in the zone mentioned to the eleventh magnitude, with as many of the twelfth as could be conveniently added without interfering with the other observations. The methods of observation and reduction, and the instruments, being in many respects new, full descriptions are given as introductory to the Tables.

25. *Chemical Examination of the Baker's Bread of Philadelphia*; by C. M. WETHERILL, Ph.D., M.D.—Dr. Wetherill observes after 24 analyses, that although he found that adulterations were employed in a very few instances, the bread is generally quite pure. Alum was detected in two samples and copper in one. Salæratus was found to be in very common use among the bakers.

26. *Report of the Commissioner of Patents* (CHARLES MASON, Esq.), *for* 1854: *Arts and Manufactures.* 776 pp., 8vo, with a separate volume of plates in 332 pages 8vo. Washington, 1855.—The Report of the Commissioner of Patents, always an important document, is not now confined merely to descriptions of the various patents issued; the illustrations, constituting a second volume, are exceedingly numerous, and although woodcuts, appear to have been made with care. Each of the 332 pages is filled with neat figures embracing views of machinery, implements of various kinds, etc., etc.,

27. *Report to the Secretary of the Smithsonian Institution on the Fishes of the New Jersey Coast*, as observed in the Summer of 1854; by SPENCER F. BAIRD, Assist. Secretary S. I. From the ninth Annual Report of the Smithsonian Institution for 1854, 40 pp., 8vo. Washington, 1855.—This valuable Report embraces various notes on the localities habits and characters of the species of fish, 67 in all, observed by Dr. Baird on the Coasts of New Jersey and Long Island.

28. *A Geological Map of Wisconsin*; by J. A. LAPHAM, of Milwaukie, Wisconsin.—This colored map, measuring 11 inches by 14, indicates in colors the outlines of the principal rock strata of the state of Wisconsin, mentioning the rocks by their western names and without indicating their eastern equivalents.

29. *Proceedings of the American Association for the Advancement of Science*, 8th Meeting, held at Washington, D. C., May 1854, 316 pp. 8vo.—To be had of Prof. J. Lovering, Cambridge, Mass., Permanent Secretary of the Association, or J. P. Putnam & Co., New York City.

EIGHTH ANNUAL REPORT of the Regents of the University of the State of New York, on the Condition of the State Cabinet of Natural History and of the Historical and Antiquarian Collection annexed thereto. Made to the N. Y. Senate, Jan. 15, 1855. 70 pp., 8vo. Albany, 1855.

Sixty-Eighth Annual Report of the Regents of the University of the State of New York. Made to the Legislature, March 1, 1855. 350 pp., 8vo. Albany, 1855. —The Appendix contains considerable meteorological information.

John Phillips: A Manual of Geology, Practical and Theoretical, with a geological map of Great Britain and numerous illustrations of fossils, scenery, &c. Crown 8vo. London and Glasgow: Richard Griffin & Co. 12*s* 6*d*.

Dr. Lindley: Ferns of Great Britain; in splendid folio, represented of life size and colored by nature-printing.—Parts I. to IV. have been issued. 6*s*. each. London: Bradbury & Evans, 11, Bouverie street.

T. C. Simon: Scientific certainties of Planetary life, or Neptune's light as great as ours, &c. Fcap. 8vo. 1855. London: T. Bosworth.

Dr. Lardner: Museum of Science and Art. Vols. I. to VI. completed; continued weekly. London: Walton & Maberly.

M. Faraday: Experimental Researches in Electricity, Vol. III. 1855. London: Taylor & Francis.

Wm. Clark: A History of the British Marine Testaceous Molluscs. 8vo. 1855. London: J. Van Voorst. 15*s*.

Mrs. Loudon, assisted by George Don, F.L.S., and David Wooster, late curator of the Ipswich Museum: New edition of Loudon's Encyclopedia of Plants. 1 Vol. 8vo., with above 12,000 wood-cuts. 1855. London: Longman, Brown, Green & Longmans. £3 13*s*. 6*d*.

Ransome's Ipswich Museum: Portrait Gallery of Celebrated Scientific Men. 60 Lithographic Portraits, 22 inches by 15. London: S. Highley. 7*s*. each; or the complete series in Portfolio, £14.

Entomologist's Annual for 1855. London: J. Van Voorst. 2*s* 6*d*.

Charles Johnson, Esq.: The Ferns of Great Britain, illustrated by John E. Sowerby. 1 Vol. 49 pl. London: J. E. Sowerby. 27*s*. colored; 14*s*. partly colored.

Jonathan Pereira: Lectures on Polarized Light; 2nd. edit., greatly enlarged by the Rev. Baden Powell. Fcap. 8vo., with woodcuts. London: Longman, Brown, Green & Longmans. £3 13*s*. 6*d*.

Dr. H. Schacht: Das Mikroscop und seine Anwendung insbesondere für Pflanzen-Anatomie. 2nd edit. 206 pp., 8vo, with 5 plates. Berlin.

Edwards & Haime: Monographie des Polypiers Fossiles des Terrains Paleoziques. 4to, with numerous plates. Paris.

Junghuhn: Fossiles de Java. Part IV (Echinodermes). Leyden: 1854.

Pictet: Materiaux pour la Paleontologie Suisse, &c. 4to, with plates.

Sur les Tempètes Electriques et la quantité de Victimes que la foudre fait annuellement aux Etats-Unis d'Amerique et a l'Ile de Cuba, par Andres Poey (de la Havane). 15 pp., large 8vo. Versailles, 1855.

Tableau Chronologique des Tremblements de Terre ressentis a l'Ile de Cuba de 1550 à 1855; par M. Andre Poey. (Extr. des Nouvelles Annales des Voyages, June 1855). 26 pp. 8vo.

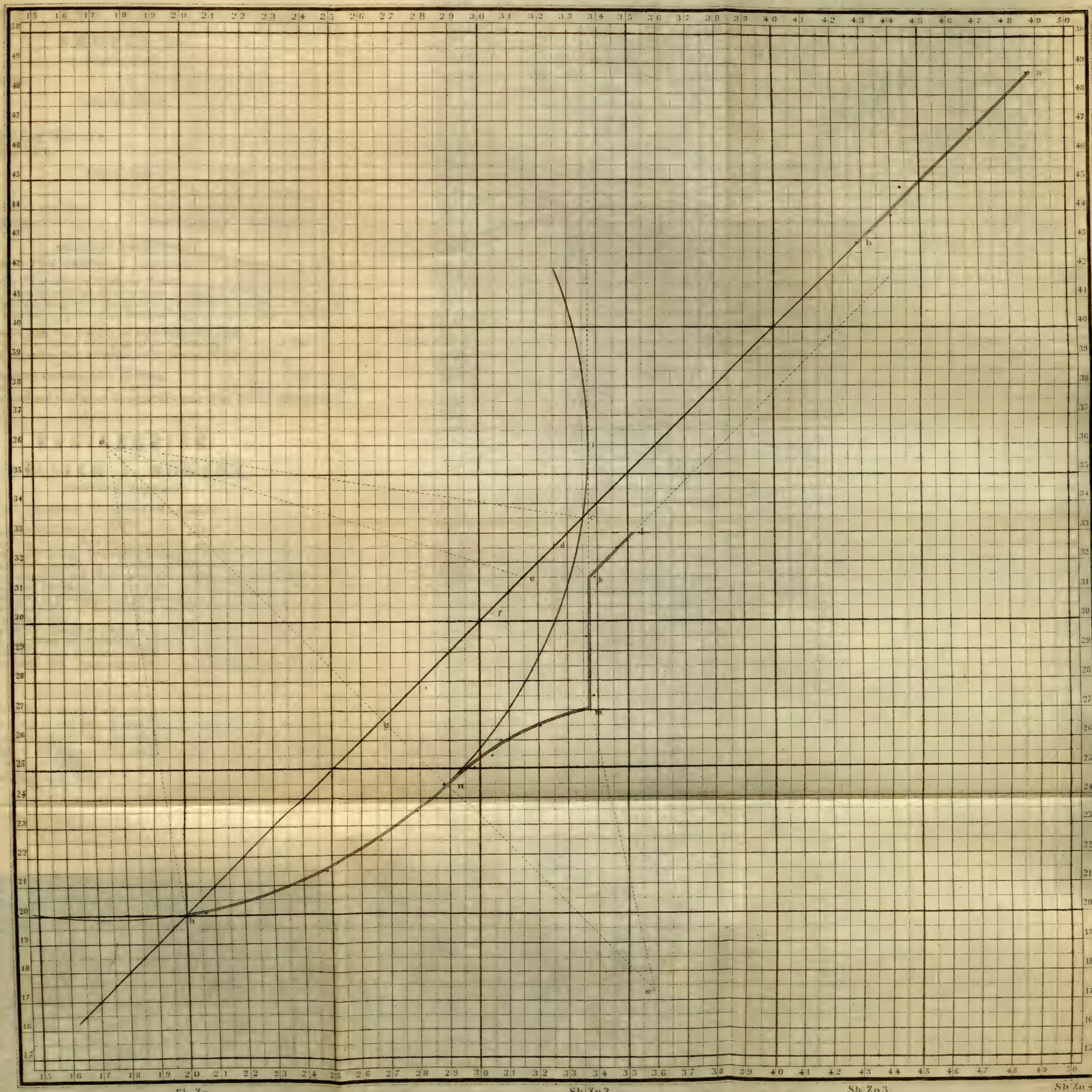
Mémoire sur la fréquence des Chutes de Grèles à l'Ile de Cuba, des cas qui eurent lieu de 1784 à 1854, et des temperatures minima de la Glace et de la Gelée blanche observées dans cette ile; par M. André Poey. 20 pp., 8vo., Paris, 1855.

The Quarterly Journal of the Geological Society, Vol. X, Part 2, May 1, 1855.

Proceedings Boston Soc. Nat. Hist. Vol. V, p. 200.—The following officers were elected in July for the ensuing year. John C. Warren, M.D., President; C. T. Jackson, M.D., and D. Humphreys Storer, M.D., Vice-Presidents, S. L. Abbott, M.D., Corresponding Secretary, etc.—p. 201, On a new locality of the so-called *Posidonomya* of the Mesozoic rocks, in Virginia; and on the effect of Trappean rocks on the same beds; *W. B. Rogers.*

Proceedings of the Acad. Nat. Sci., Philadelphia, Vol. VII, No. ix.—p. 340, Description of a new Mollusc from the Red Sandstone near Pottsville, Pa., (Cypricardia Leidyi), with a plate; *Isaac Lea.*—p. 342, Habits of the Moose; *C. J. Gilman.*—p. 346, Notes on the Amaræ of the U. States; *J. L. LeConte.*—p. 356, Synopsis of the Hydrophilidæ of the U. States; *J. L. LeConte.*—p. 375, Descriptions of some of the new Marine Invertebrata from the Chinese and Japanese seas; *W. Stimpson.*

CURVE EXHIBITING THE VARIATION IN COMPOSITION OF THE CRYSTALS FORMED IN THE ALLOYS OF ZINC AND ANTIMONY.



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THE
AMERICAN
JOURNAL OF SCIENCE AND ARTS.
[SECOND SERIES.]

Art. XXVII.—*On the Eagre of the Tsien-Tang;* by D. J. Macgowan, M.D.*

No one who has witnessed under favorable circumstances, an Eagre,† or the tidal phenomenon called from its Indian designation a "Bore," can be surprised at the account given by Arrian of the tide near the mouth of the Indus, or at the astonishment manifested by Alexander and his army when they first beheld its action. On the Hooghly branch of the Ganges, where I first saw the Bore, it presented no feature calculated to excite wonder; but on the Tsien tang, it struck me as being one of the grandest spectacles in nature.

A Chinese Proverb reckons three wonders of the world,—the tides at Hang-chau, the thunder in Lung-chau,‡ and the demons

* From the Transactions of the China Branch of the Royal Asiatic Society.—Read to the Society, 12th January, 1853.

† "His manly heart, whose noble pride
Was still above
Dissembled hate or varnish'd love,
Its more than common transport could not hide;
But like an *eagre* rode, in triumph o'er the tide."—*Dryden.*

‡ Lungchau, (thunder district) is a long mountainous peninsula in Canton province opposite the island of Hainan, and is celebrated throughout China for several myths respecting its thunder storms, which doubtless reverberate through the alpine regions of that latitude in a manner to awaken awe and superstition. Standard Encyclopædias, quoting from various authors on the subject, inform us, that after thunder storms black stones are found, emitting light and a sonorous sound on being struck. At times, also, hatchet-shaped things are picked up which are useful amulets, The

at Tang-chau.* To the first named marvel, the province of Chih-kiang owes its name: that it has not been mentioned in the Travels of Marco Polo, is to be accounted for from the circumstance, that the phenomenon did not occur during any of his visits to the city of Hang-chau: otherwise it would not have remained until the present day undescribed, nor would a distinguished geographer have stated, that "In the North Pacific we have neither the bores of a Hooghly nor the terrific tides of a bay of Fundy."†

The Tsien-tang river, originally called Chih-kiang, takes its rise in a mountainous region where the provinces of Kiang-si, Fuh-kien, and Chih-kiang are long conterminous, and after pursuing a northeasterly course, disembogues into the Hang-chau Bay, a short distance below the city of that name. It is about 220 miles long, flowing more than two-thirds of its way through a mountainous country; the remainder, draining a low alluvial plain. A considerable quantity of silt is conveyed by it to the sea, thus aiding the Yang-tsz' and Yellow Rivers in extending this part of the continent eastward. Being broad, shallow, and rapid. it is navigated with difficulty by flat bottomed vessels, which seem admirably constructed for that purpose. They are drawn by boatmen against the stream, with ropes several hundred feet in length attached to the topmast, those on board being engaged in poling. At Hang-chau, it is about four miles wide; and at its mouth, about ten miles distant, the width is seven. The configuration of a river, however, has little effect in giving rise to Eagres, the cause of these phenomena must be sought for in the form of its estuary, where is presented in a remarkable degree the peculiar features necessary to a sudden elevation of the tidal wave,—a shallow and gradually contracting bay, subject to high tides, at the embouchure of a river more or less obstructed by sands.

fields are often furrowed by thunder as if they had been ploughed. In a temple consecrated to the Thunder Duke, the people annually place a drum, drawn thither on a carriage purposely constructed, which it is supposed he beats during a storm; and it is said, that since a drum covered with paper has been substituted for one covered with leather, the peals of thunder have been less severe. Formerly the drum was placed on the top of a mountain, and a boy left there as an attendant on the thunderer—a sort of sacrifice to him.

* Tang-chau is not far from the birth-place of Confucius, a peninsula of the Shantung Promontory. Thither it is believed the spirits of the departed repair, until summoned to judgment in Hades. Innumerable accounts are given of the pranks of ghosts, which are for a season at liberty in that region. The "ghost's lantern," *ignis fatuus*, is often seen there.

† Vide *Johnston's Physical Atlas of Natural Phenomena*. This splendid and usually accurate work contains a few slight errors in treating of this *terra incognita*. The lofty mountain chain passing through the entire length of Formosa, is omitted in the Chart of the mountain chains of Asia. A very large space in the Geological Chart should have been colored to represent the extensive district of pure alluvium between 30.15 and 35. N. extending from the sea a great distance comprising an area of about eighty thousand square miles, destitute even of a pebble or grain of sand.

Hang-chau Bay, (a foreign designation, Chinamen not distinguishing it from the sea,) is an indentation in the extreme eastern part of the Chinese coast, where the continent converges towards the Pacific. as if to form a cape; its northern headland is Yang-tsz' Cape, an alluvial flat, formed by the great river of that name; the opposite headland is of a very different character, being formed by the termination, at Ketow Point, of the Nain-ling chain of mountains. Thus, on one side the shore is so low, and the bay so shallow as to render it perilous to navigators, while at the other, there is a bold coast and deep water. From Ketow Point to Yang-tsz' Cape, the distance is sixty miles, which is the width of the estuary. A line intersecting it, drawn from the mouth of the river, would be about seventy-five miles. A portion of the Chusan Archipelago is included in the southeastern part of the bay. Although these islands are generally separated by deep channels, the descent of the submarine ground on this part of the coast is extremely gradual, owing to the ever accumulating detritus of the great rivers. The bay itself, is very shallow, and constantly decreasing in depth, the channel being wholly obliterated towards the Tsien-tang. At its southern boundary it receives the water of the Ta-hiah or Ningpo river, where the tides are in no respect remarkable; and to the north of this, near the embouchure of the Tsien-tang, Chang-ngo is disembogued, in which river the tides have a great and rapid rise and fall.

Navigators are well acquainted with the force of tidal currents among the Chusan islands, and in the other part of the bay; there is a rapid increase of their velocity as you advance up the funnel-shaped frith, where they rush at sixteen miles an hour. In the neighborhood of Chapoo, where they are more moderate, their flow is between eleven and twelve knots, with a fall of twenty-eight feet. Vessels approaching this part of the coast are thus in danger of being lost among sands without sighting the low shores. Captain Collinson, when endeavoring to find a channel to Hang-chau in the H. C. Steamer *Phlegethon*, experienced a tide of "eleven and a half knots, when nineteen miles distant from the Chapoo Hills, and two from the shore. Traversing the river [estuary] which at this point is about fifteen miles wide, there was no continuous channel found, although there were some deep spots. When the *Phlegethon* was exposed to this tide, she had an anchor down, with a whole cable, (having previously lost an anchor and cable in endeavoring to hug up,) was under her full power of steam, with sails set, and was still driving." Whales are not unfrequently drawn in by this vortex, and left floundering on the strand or in shallow water by the receding wave; some of the accidents recorded of the unfortunate *Cetacea* are worth quoting. * * *

At the upper part of the bay, and about the mouth of the river, the Eagre is scarcely observable, but owing to the very gradual descent of the shore, and the rapidity of the great flood and ebb, the tidal phenomena even here present a remarkable appearance. Vessels, which a few moments before were afloat, are suddenly left high and dry on a strand nearly two miles in width, which the returning wave as quickly floods. It is not until the tide rushes beyond the mouth of the river, that it becomes elevated to a lofty wave constituting the Eagre, which attains its greatest magnitude opposite the city of Hang-chau. Generally there is nothing in its aspect, except on the *third* day of the second month, and on the *eighteenth* of the eighth, or at the spring-tide about the period of the vernal and autumnal equinoxes, its great intensity being at the latter season. Sometimes, however, during the prevalence of easterly winds, on the *third* day after the sun and moon are in conjunction, or in opposition, the Eagre courses up the river with hardly less majesty than when paying its ordinary periodical visit. On one of these unusual occasions, when I was travelling in native costume, I had an opportunity of witnessing it, on December 14, 1848, at about 2 P. M.

Between the river and the city walls, which are a mile distant, dense suburbs extend several miles along the banks. As the hour of flood-tide approached, crowds gathered in the streets running at right angles with the Tsien-tang, but at safe distances. My position was a terrace in front of the Tri-wave Temple, which afforded a good view of the entire scene. On a sudden, all traffic in the thronged mart was suspended, porters cleared the front street of every description of merchandise, boatmen ceased lading and unlading their vessels, and put out into the middle of the stream, so that a few moments sufficed to give a deserted appearance to the busiest part of one of the busiest cities of Asia. The centre of the river teemed with craft, from small boats to huge barges, including the gay "flower boats." Loud shouting from the fleet announced the appearance of the flood, which seemed like a glistening white cable, stretched athwart the river at its mouth, as far down as the eye could reach. Its noise, compared by Chinese poets to that of thunder, speedily drowned that of the boatmen; and as it advanced with prodigious velocity,—at the rate, I should judge, of twenty-five miles an hour,—it assumed the appearance of an alabaster wall, or rather of a cataract four or five miles across, and about thirty feet high, moving bodily onward. Soon it reached the advanced guard of the immense asemblage of vessels awaiting its approach. Knowing that the Bore of the Hooghly, which scarce deserved mention in connection with the one before me, invariably overturned boats which were not skillfully managed, I could not but feel apprehensive for the lives of the floating multitude: as the foaming wall of water

dashed impetuously onwards, they were silenced, all being intently occupied in keeping their prows towards the wave which threatened to submerge every thing afloat; but they all vaulted, as it were, to the summit with perfect safety. The spectacle was of greatest interest when the Eagre had passed about one-half way among the craft. On one side they were quietly reposing on the surface of the unruffled stream, while those on the nether portion were pitching and heaving in tumultuous confusion on the flood, others were scaling with the agility of salmon, the formidable cascade. This grand and exciting scene was but of a moment's duration,—it passed up the river in an instant, but from this point with gradually diminishing force, size, and velocity, until it ceased to be perceptible; which Chinese accounts represent to be eighty miles distant from the city! From ebb to flood tide, the change was almost instantaneous; a slight flood continued after the passage of the wave, but it soon began to ebb. Having lost my memoranda, I am obliged to write from recollection: my impression is, that the fall was about twenty feet; the Chinese say that the rise and fall is sometimes forty feet at Hang-chau. The maximum rise and fall at spring tides is probably at the mouth of the river, or upper part of the bay, where the Eagre is hardly discoverable: in the Bay of Fundy, where the tides rush in with amazing velocity, there is at one place a rise of seventy feet; but there the magnificent phenomenon in question does not appear to be known at all. It is not, therefore, where tides attain their greatest rapidity, or maximum rise and fall, that this wave is met with, but where a river and its estuary both present a peculiar configuration.

Dryden's definition of an Eagre, appended in a note to the verse above quoted in the *Threnodia Augustalis*, is, "a tide swelling above another tide," which he says he had himself observed in the river Trent.—Such, according to Chinese oral accounts, is the character of the Tsien-tang tides,—a wave of considerable height rushes suddenly in from the bay, which is soon followed by one much larger. Other accounts represent three successive waves riding in; hence the name of the temple mentioned, that of the Three Waves. Both here and on the Hooghly, I observed but one wave; my attention, however, was not particularly directed to this feature of the Eagre. The term should perhaps be more comprehensive, and express "the instantaneous rise and advance of a tidal wave;" the Indian barbarism "Bore" should be discarded altogether.

A very short period elapsed between the passage of the Eagre and the resumption of traffic; the vessels were soon attached to the shore again, women and children were occupied in gathering articles which the careless or unskillful had lost in the aquatic meleé. The streets were drenched with spray, and a considerable

volume of water splashed over the banks into the head of the grand canal, a few feet distant.

The earlier records of the Chinese would seem to indicate that the beauties of nature had failed to make deep impressions on their imaginations; but from a remote period, men of letters have portrayed nature with true poetic feeling, and in delineating landscape they have particularly excelled. An object, therefore, of such sublime grandeur as the Eagre, occurring frequently at the Athens of China, could not fail to attract the pencil of the literati; it has therefore often been the theme of poetic scholars. No one, not himself capable of clothing the "perception of similitude in dissimilitude," as Wordsworth expresses it, should attempt a version of their artistic effusions; yet an account of this tidal phenomenon would be extremely defective, unless it were presented, to some extent, from a Chinese point of view.

It has been already mentioned, that the Chinese regard the Eagre as one of the wonders of their world, and that it gave name to the province. As might be expected, therefore, it is blended with their mythology. It is not a little remarkable, however, that it should be popularly ascribed to the spiritual energy of a *shin* (or god,, who lived so recently as five hundred and forty years before our era, or about twenty years before the birth of Confucius. At that period the Tsien-tang was the boundary of two belligerent kingdoms, Wú and Yueh. Fú Chai, king of the former, incensed against his minister Wú Tsz'si, for opposing the terms of a treaty, submitted by Chung, ambassador of Kü Tsien, king of Yueh, sent him a sword, with which, understanding his master's will, he committed suicide by cutting his throat, a method still pursued by sovereigns in China towards officers of distinction who have incurred their displeasure. This incident in ancient history is recorded in the spring and autumn annals of Confucius; but in a work, entitled "Spring and Autumn Annals of the States of Wú and Yueh," a historical novel, written several hundred years after, a prevailing myth is superadded to the authentic narrative, which the author himself seems to credit, and which to the present day is received as verity.

Wú Tsz'si's corpse, which was thrown into the Tsien-tang, after being carried to and fro by the tide for some time, tunnelled a passage through the hills on the Yueh side, as far as the tomb of the quondam ambassador Chung, whose cadaver he took with him to the estuary. Since that period, it is stated Wú Tsz'si has been the god of the Eagre, his periodical indignation being exhibited by its violence; hence the sacrifices and prayers officially presented at appointed seasons to propitiate his anger. Monarchs of almost every dynasty have honored him with titles, so that they fill half a page of the work in which they are recorded.

Since the apotheosis of the faithful statesman, their penchant for hero-worship has led the people to multiply the tutelary divinities of the tide to a great extent. Thus it has lords many and gods many, so much so that a Pantheon has been erected at Haining, the Temple of the Tidal gods, in which, besides a central image of Wú Tsz'si, there are fifteen others ranged in rows on either side. Some of the most distinguished of these *shin* have temples dedicated to their special honor, and therefore deserve notice in this sketch.

The Tidal King Temple, is in the country. "Its *shin* (or god) was an officer named Stone," who, in 828 A. D., undertook the restoration of a dyke, which an Eagre of unusual violence had overthrown; and failing in the construction of the foundation, drowned himself from chagrin. "He afterwards became a *shin*," (or god,) and three centuries later, on the occasion of a combat between the people and rebels, who were attempting to capture Hang-chau, his name was seen inscribed on a streamer in the darkened sky, where also unearthly noises were heard. The enemy instantly succumbed. Again, a hundred years later, when an extraordinary Eagre destroyed the dykes, occasioning great loss of life and property, the waters retired through prayers and sacrifices to his manes. In consequence of this, a Budhist priest was appointed to the charge of his temple, that a regular religious service might be kept up to his honor.

The Temple of Unanimity. "Its *shin* is Luh kwei," a military officer who flourished in the twelfth century. He fell on the field of battle, and "became a god" Not long after, when the dykes were overthrown by a violent tidal wave, his *shin* led soldiers from Hades and forced back the tide. Subsequently, when it returned with greater force, extending to the city walls and flooding the country, he re-appeared accompanied by three damsels bearing streamers. By their united labor, the stones, which had been carried away, were brought back for a foundation. To commemorate these services, the title of Great Mound Earl, was conferred upon him by the Emperor, who also ennobled the fair attendants. The temple has, besides several stone images, those of the three fairies and twelve horary *shin* (or gods), for alternate officers in the government of the tide.

One more example will suffice: It is of a temple some distance from the city, erected in honor of a tea merchant, who, above seven hundred years ago, devoted a large part of his property to the construction of a dyke, which was overthrown soon after its completion. In despair he cast himself into the swelling wave, which wafted his corpse from place to place; wherever it rested, sand was thrown up, and thus a foundation was provided for another embankment. Posthumous titles were conferred upon him, and a temple erected in which divine honors are paid him.

Despite the superstitions of the populace, which had the sanction of government, there have been philosophical minds, who, without discarding popular myths, speculate on the natural cause of the phenomenon, and propound theories for its explanation. From a summary of these, contained in the Topography of the Prefecture, the following, having reference also to tides in general, have been selected :—

1st, A consequence of the contracting and expanding of the Spring of Nature.

2nd, An enormous Serpent entering into and emerging from oceanic caverns causes their ebb and flow.

3d, The Budhist theory, which attributes them to the transformations of the Divine Dragon.

4th, A consequence of the Sun rising out of the ocean.

5th, From being influenced by the Milky Way.

6th, From being drawn by the Sun.

7th, From the phases of the Moon. According to Plutarch, Pytheas of Marseilles first ascribed tidal oscillation to this orb.

8th, Pulsation of the Earth's blood—water.

9th, Expansion and contraction of the Atmosphere under the influence of the heavens, which occasion a corresponding rise and fall both of land and water: thus, when the waters flood, the earth ebbs, and *vice versa ;* of which landsmen are no more conscious, than persons in a boat are of the tides. In confirmation and illustration of this theory, the fact is adduced, of the rise and fall of the pilous surface of the dried skins of certain sea animals simultaneously with the tide!

By others, the reciprocal action of the dual powers was referred to, as explaining all tidal phenomena. One, like Pliny, says the sun and moon draw the waters after them.

Some, however, more shrewd than the rest, sought for the cause of the peculiar tides of the Tsien-tang, in the configuration of its embouchure, an idea which is combatted by another writer as untenable, because the Ningpo and Chang-ngo river present the same features (which is a mistake), and have no Eagre. It is owing, says this author, to the greater quantity of water which comes into that river. The learned adhere to one or the other of these theories, the populace to that of the serpent, all classes believing that the gods already named are actively concerned in the matter. It should be remembered that until Newton cleared up the subject, very crude notions prevailed respecting it in Europe.

Although the inhabitants of this region are the most timid in the empire, there were among them, at one period, many who from mere bravado would plunge into the frightful wave. A proclamation against the custom still exists, from which it appears that many lives have been lost in attempting this exploit. After appealing to that regard which all should entertain for the body

inherited from their parents, the document exhorts all good people not to go voluntarily to the dread abode of the Sea Dragon; and admonishes them that their souls, should they perish in this manner, will sink into eternal perdition under the Nine Springs of Hades. "Life has its natural limits, why not await the last day fixed by the decree of Heaven? Parents, wife, and children, will be seen gazing from the shore, bewailing your fate, and no condolence can be offered them." Condign punishment is threatened against all who should disregard the admonitions.

The geological changes resulting from these great tides are less than might be expected, owing to the degradation of the land by tidal and fluviatile action elsewhere. Occasionally the land suffers the loss of considerable portions of soil, but it is speedily replaced by the same power, and is ever extending, by accumulations of detritus. There have however been some marked physical changes effected within the historic period, by the combined action of the Eagre, and of the two great rivers of China.

Until about the eleventh century, there was a ledge of rocks projecting above the surface of the stream, near Hang-chau city, known as *Losah* rock, a Sanscrit term employed by Budhists to express *devilish*. Hence that part of the Tsien-tang, taking its name from this obstruction to navigation, was once called the Devilish River. It was large enough to be a place of resort on certain festivals—religious and Thespian. A poet of that period playfully inquires how it happened that the great Yú, whilst opening the river, omitted to perforate a rock a *le* in length. Finally it yielded to the attack of the fierce Eagre, and gradually disappeared.

A smaller rock measuring only 16 feet high, 14 long, and 6 wide, disappeared 145 B. C. Two years after, it was brought into sight again by the same force which had removed it, when it disappeared altogether.

Kam-pu, once a mart of considerable importance, the port of what, at one period at least, was the metropolis and capital of the empire, mentioned by Marco Polo as an extremely fine port, twenty-five miles from the city, frequented by all the ships, that bring merchandise from India, is now an insignificant walled village, in consequence of the receding of the sea, and the filling up of the bay. More than fifty miles distant, another city, Cha-pu, has sprung up near the sea, which, for a long time to come will be the port of Hang-chau; but the Yellow River and the Yang-tsz' are surely, and not slowly, depositing silt, which will render it in its turn inaccessible.

Chinese ingenuity has long been exerted, but with indifferent success, in preserving their alluvial plain from the wasting action of the Eagre. The earliest mention of dykes is about the commencement of our era, when an engineer named Tsau, under-

took the work. Offering a thousand copper cash for every bushel of earth brought for the embankment, he induced laborers to collect in immense numbers, who on being required to bring stones instead of earth, soon disappeared. From this circumstance the work was called Tsien-tang, the *cash* or *money* dyke—a name subsequently applied to the district, and finally to the river, its present designation.

Embankments of earth were generally resorted to, until the middle of the seventh century, when stone was employed; but the frequent inundations which occurred from the tide, shew that the art of constructing dykes was imperfectly understood. The general statistics of the empire ascribe the first erection of dykes to the Prince Wú Shuh, 930 A. D., whose court was at Hang-chau. This Viceroy, according to the records of the Sung dynasty, also employed intimidation and cajolery in defending the land from the violence of the Eagre. First he summoned five hundred daring archers, each of whom was provided with six arrows, which they let fly at the aqueous monster as it came swelling onwards; next he made prayers and offerings in the temple of Wú Tsz'-si; and finally, he composed a hymn, which he enclosed in an envelop, together with the key of the dyke water-gate, and addressing it to the gods generally, threw it into the river; forthwith the waters retired. Frequent reference is made to these proceedings in topographical and other works. Perhaps they were performed merely for the amusement of the people, or, if he had any faith in them, he was too sagacious and practical to confide in anything less than dykes, superior and altogether different from those heretofore employed. Bamboo frames were made and sunk on the banks behind a row of piles, and filled with large stones; and on the foundation thus laid, the embankment was built. To the present day no method has been found superior to that of this prince. The contrivance is not unlike that which western engineers have found most effectual in protecting river banks.

This work endured only sixty years; a tide carried much of it away, laying the country under water, and occasioning devastation. An imperially appointed officer, aided by the military, was occupied seven years in vain attempts to construct a permanent embankment. At length he was dismissed, and his successor, by employing the method mentioned above, completed the work in one year. It however was not owing to superior skill or energy that he accomplished it, but to one of those sudden shifts of the loose bed of the estuary, which have given rise to so many superstitions. According to the statement he made to the emperor, who had expressed a desire to know how the work had been so quickly finished, the engineer, after praying to Wú Tsz'-si, had the satisfaction of procuring his aid; for the next day the waters retired, having raised several miles of yellow sand, which ena-

bled him to accomplish the undertaking. Nevertheless disasters, from the washing away of embankments when a violent storm occurred simultaneously with the Eagre, are recorded during every reign. It was found necessary to have soldiers stationed on its entire length to mark the first point of loosening.

In 1026 A. D., an officer named Tsiang constructed a stone dyke several miles in length, of such excellence and strength, that about seventeen years after the grateful people of Hang-chau erected a temple to him on the embankment.

The emperor Kau-tsung, in 1131, ordered ten iron plates, each weighing above one hundred and thirty pounds, to be employed as charms against the mischievous spirits, in hopes that they would thereby be induced to respect the dykes, which were kept up at so much cost. They were sunk at convenient distances, in front of the embankments; but on the following year, iron charms and stone walls were all carried off, and the loss of life and property was unusually great in consequence. On the following year, the experiment was again tried, the monarch himself writing the charms, according to the forms of the Tauist sect. These iron plates were deposited in chests of choice wood workmanship, and sunk by Tauist priests with great ceremony. The result is not mentioned, but the record goes on detailing numerous repairs following successive disasters.

During the period of the Mongolian reign in China, the destructive action of the tides was remarkable. Kublai, the first emperor of that, the Yuen dynasty, caused repairs to be made; but they could not have been effective, as soon after it is recorded that more than twenty thousand men endeavored in vain to restore the breaches of a few miles. Disasters occurred every year, and repairs were going on incessantly; at length a Mongolian officer (judging from his name) informed the emperor Tai-ting of the experiment made with Tauist charms; and as his majesty approved of the propsition for repeating them, they were doubtless tried.

Just as the Yuen dynasty was drawing to its close, the reigning emperor called upon the head of the Tauist sect to perform his arts to stay the flood; and on the failure of these, he had two hundred and sixty Budhist priests engaged in trying the virtue of their ceremonies for the same purpose, and with the same result. At length after a general consultation among imperial officers, (men who had displayed ability in constructing the Grand Canal,) the whole population was either called out to labor, or required to subscribe funds. Budhists and Tauist priests, it is stated, were not exempted. Embankments, were accordingly constructed, superior to any which had preceded them; and to commemorate the event of their completion, the name of the city In-kwan, at the upper part of the estuary, was changed to Ning-hai, or the "sea tranquillized."

No extensive repairs were called for until twenty years after, but from that time, the fall and rebuilding of dykes are frequently mentioned. After one of these disasters, an ox was sacrificed to the *shin* of the eastern sea, the repairs at this time required thirteen years' labor, and nearly the whole province was called on to aid in defraying the expense.

A typhoon, in 1578, which lasted three days and nights, caused with the tide extensive losses. Roaring or howling noises are mentioned several times in connection with sea storms; they were peculiarly alarming in that typhoon. Salt water covered the fields for many miles, drowning many of the inhabitants. In reconstructing the dykes, unusual care was taken to have stones hewn in such a manner as to afford mutual attachment and support: five were laid lengthwise, and on these, five more were placed crosswise; the wall was then carried up, the stones, which were five feet two inches long, and one foot eight inches in thickness each way, being placed alternately sideway and endwise towards the stream. Some details are given of the cost of these stones. The quarrying and dressing was three mace of silver, one mace for boat-hire and seven candareens for portage,—that is, about seventy-five cents for each stone. The dykes which I saw on the Tsien-tang were perpendicular walls; the Chinese, however, are perfectly aware that a slope towards the water gives additional strength to an embankment; a sea wall at one part of the bay near Chin-hai, is placed a portion of its way at an angle of thirty degrees.

In the reign of Kang-hi in 1665, after a typhoon, like the former of three day's duration, and occurring with an Eagre, the dykes were generally overturned, with much destruction of life and property. In rebuilding them, iron clasps were employed to keep the stones together. This illustrious monarch personally inspected the works, and gave many directions respecting them. During this reign one of those phenomena occurred, which has at different periods in their history struck the people with amazement. Officers and people were bewailing an inroad of the tide which had carried off several miles of country, and were sacrificing to the gods for succor, when another tide suddenly brought the whole back again!

Kang-hi's successor Yung-ching (1723–36,) endeavored by the use of huge amulets and numerous religious ceremonies to appease the gods, although he did not neglect the embankments. Finding that five iron oxen which had been placed at different points of the river and estuary were not likely to prove efficacious talismans, he ordered the erection, at his own expense, of a great temple dedicated to the *Hai-shin* or sea gods, in the city of Haining. A stone slab or monument contains the imperial rescript in relation to the structure. "I find," says his majesty, "that

the ancient sages sacrificed to the gods of the hills and springs, on account of their power of delivering and succoring men; it is proper therefore to seek their favor. They should be revered and recompensed; men's hearts should be excited to regard them with reverential awe, lest they should gradually presume to slight them. In the second year of this reign, when the dykes were overthrown, I sent officers with funds to examine and restore them. I apprehend that the people in general are wanting in reverence for the *shin ming* [gods], and that they are often wanting in piety; I therefore urge you to decorous and respectful demeaor in everything that regards them, and hereby enjoin on the officers to impart instruction to each family, in order that all may be aroused and quickened. Of late years the gods have silently vouchsafed protection, enabling the people to repose in safety, and though this year the tides have been very great, threatening general ruin, yet they happily abated without damage to the embankments, entirely in consequence of the mysterious protection which the gods, having regard for us all, have vouchsafed. I therefore make a special gift from the privy purse of one hundred thousand taels of silver (about $130,000) for the erection of a sea god Temple, that they may therein be honored and recompensed. Let the civil and military officers measure off the ground, and engage men to rear a spacious and magnificent edifice, in which to honor, and sacrifice to, the gods, in acknowledgment for their protecting the people and warding off calamities. Its appearance, moreover, will serve to affect to gratitude those from afar, as well as those who are near, causing them to discard impiety and to become reverential."

In the same spirit the succeeding emperor Kien-lung addressed the people in 1758. Referring to a visit he had made to Hang-chau, when he witnessed the Eagre, he says "surely the protection which the *shin ming* have vouchsafed the city, (since the repairs of the previous reign,) requires the erection of a temple to the gods of the sea, in the capital of the province," and then directs that their power and virtue should be honored accordingly. On another occasion he devoutly thanks the *shin*,* for the favor experienced, and states that he keeps it constantly in mind.

From the foregoing extracts from native authorities it will be seen, that the people of this part of China are in an analogous position to the Hollanders. The subject of dykes has long occupied their attention, and tasked their energies,—on the Hwàng, to resist fluviatile, and on the Tsien-tang, tidal action: volumes have been written on the subject, and a corps of officials are employed specially for observing the wear of embankments.

* *Shin*, is the term adopted by students of Chinese to designate God in this language, conceiving it to be the generic name of the deities of China. Accordingly throughout this paper, *Shin* has been thus translated. It should be added, however, that a majority of the older scholars concur in rendering it by *spirit*, employing the title of *Shang-te* for the chief object of worship,—the *Theos*.

To give an idea of the expense entailed by the tides of Hang-chau Bay, I have cast up the sums disbursed at different times during the reign of Kien-lung, comprising a period of sixty years (1736–96) as detailed in the Topography of Hang-chau.

For the embankment in Hang-chau district:—

	Taels of Silver.
Contributed by officers, ...	4,338,064
Appropriations from Imperial Treasury,	942,800
Contributed by the people, ...	790,526
	6,713,390

or about $130,000 per annum, but as silver is now 20 per cent dearer than it was in that reign, a corresponding reduction should perhaps be made from this sum. There is a coast line of about 180 miles, extending from Chin-hai to Hang-chau which is not included in the above; also the shore of the estuary west of Cha-poo, has more or less embankment, to the boundaries of Hang-chau, which requires large outlays. As the greatest force of the tides is experienced on the left, or Hang-chau side of the estuary, it is probable that an expenditure of one-third of the above sufficed to preserve the country from inundation. As the Yellow River is "China's sorrow," so the "difficulty" of Chih-kiang is the Eagre of the Tsien-tang.

ART. XXVIII.—*Observations on Binocular Vision;* by Professor WILLIAM B. ROGERS.

(Continued from p. 220.)

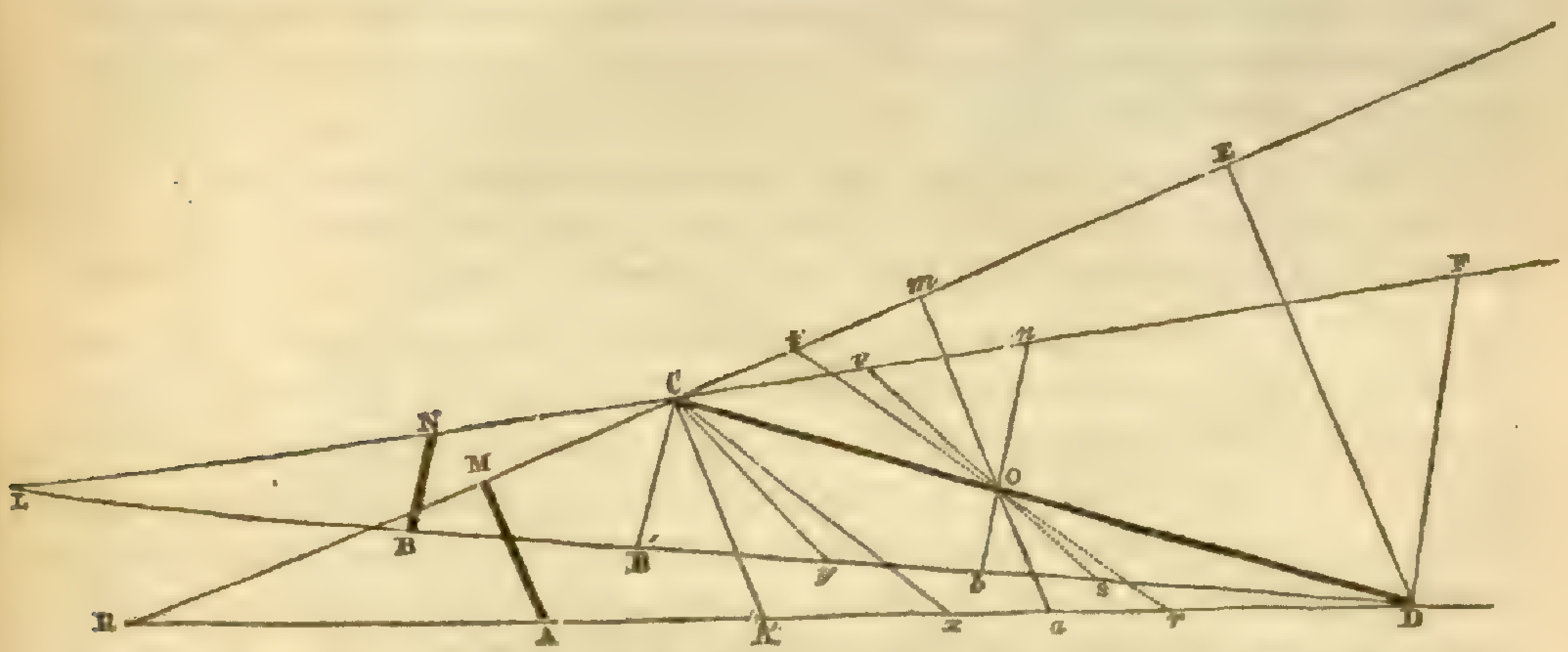
15. *On the apparent relief of the component lines as bearing on the theory of binocular perspective.*

In describing the different phases presented in the combination of inclined lines, special mention was made of the perspectiveness of the intersecting lines of which the resultant is usually formed, but certain features of the phenomenon were purposely omitted as being unessential to the general account. The following observations are intended to trace more precisely the development of the effect and in connexion with other analogous phenomena to furnish suggestions as to the origin in such cases of our perception of relief.

We have seen that when we confine the eyes to any particular stage of convergency, the intersecting lines of the resultant present themselves in a plane parallel to the paper, but when we allow them within certain limits to vary their convergency, these lines assume more or less relief. We have now to add the important fact that *while the lines thus acquire perspectiveness they become longer and more acutely inclined to one another.*

These changes usually go on until the lines assume the greatest amount of relief and the least angle of intersection of which from their positions in the diagram they are susceptible, and then the opposite changes supervene. If but slightly inclined in the diagram, they acquire the perspective position almost as soon as they are brought to intersect. Their inclination to the plane of the paper then rapidly augments, they grow longer and the included angle lessens, until in many cases it becomes imperceptible, and for a moment the two lines seem to melt into a single resultant. But as we look at this we see it again resolving itself into two intersecting lines varying continually in apparent length, mutual inclination and amount of relief. The relation of these various appearances to one another and to the visual conditions in which they occur will be more clearly understood by referring to fig. 27.

27.



In this diagram R and L denote the centres of the right and left eyes AM, and BN, the two lines which are to be combined, and RCED LCFD the planes in which the axes of the right and left eyes respectively revolve while directed to successive points of AM, BN. Since CD is the intersection of the two optical planes, it is obvious that the axes directed to corresponding points of AM BN will always meet in that line. Hence A and B will be seen united at D, M and N at C, and so other points of AM, BN will be combined at some determinate place in CD. This line is therefore the locus of all the points due to the binocular combination of the corresponding points of the component lines AM BN.

If we begin our observations by converging the axes through M and N to C and hold them in this inclination, AM and BN will appear in the position and of the magnitude represented by A′C B′C. If then reducing the convergence of the axes we direct them through A and B to D, the components will appear in DE DF. The resultant figures A′C B′ and DEF being in planes parallel to that of the diagram AM BN, their lines will

have the same mutual inclination in each case as that of AM and BN, although of greater length in proportion to their greater distance from R and L. Lastly, suppose the axes to be steadily converged through some other corresponding points of the component lines so as for example to meet at O, we shall then have in place of AM, BN a resultant consisting of the two intersecting lines *am bn* parallel to the former and lying in a parallel plane. Thus to whatever point of CD we may direct the eyes with a steady convergence, the binocular resultant will be seen to consist of two lines intersecting in that point, parallel respectively to the lines of the diagram and of length greater in proportion to their distance.

When however the eyes are allowed to vary their directions so as to combine a number of contiguous points of the component lines in rather quick succession, the resultant intersecting lines will be seen to assume a perspective attitude, so that, taking O for example as the point at which we note their momentary intersection, they will appear in some such positions as *rt sv*, presenting a perspective resultant of which the intersecting lines are longer than *am bn* and inclined to one another at a less angle. When the combination of contiguous points is spontaneously effected with great celerity the lines *rt sv* are seen to approach the position CD and appear even momentarily to coalesce with one another. But as soon as the axial movement is retarded, they pass by rapid gradations to an attitude similar to that of *rt sv* or to that of even less relief, or more nearly approaching that of *am bn*. The same effect is often apparent when the lines of the resultant are seen merely united at the ends without crossing one another, as where the point of intersection of these lines, or which is the same thing, the point of axial convergence, after moving along the contiguous part of CD reaches C or D. In this case we may observe the resultant lines in the position C*x* C*y* or some corresponding attitudes in reference to D.

It can hardly be doubted that the variable and seemingly capricious phenomena just described are dependant on the successive convergence of the axes to different distances, and on the perception resulting from this of a greater or less perspectiveness in the direction of the series of resultant points. It may be assumed that a certain degree of rapidity in effecting this succession of combinations, in other words, this successive intersection of the two lines at differents points of their length and at different distances from the eyes, is necessary to create a distinct and strong perception of relief. For as we have seen, every pause in this movement of the axes is accompanied by a reduction of the relief, and if continued destroys it altogether. Now as the progressive change of convergence of the optic axes in these experiments has the effect of carrying the intersection of

the resultant lines (O for example) along these two lines in the same direction at the same time and with equal velocity, it would seem to be a natural consequence that the feeling of perspectiveness should connect itself equally with both these lines when observed in any momentary phases of intersection. According to this view the continual changes of apparent relief and the corresponding approach or separation of the intersecting lines, would arise from differences in the force and precision of the perspective idea, depending on the greater or less rapidity of the converging movement of the eyes.

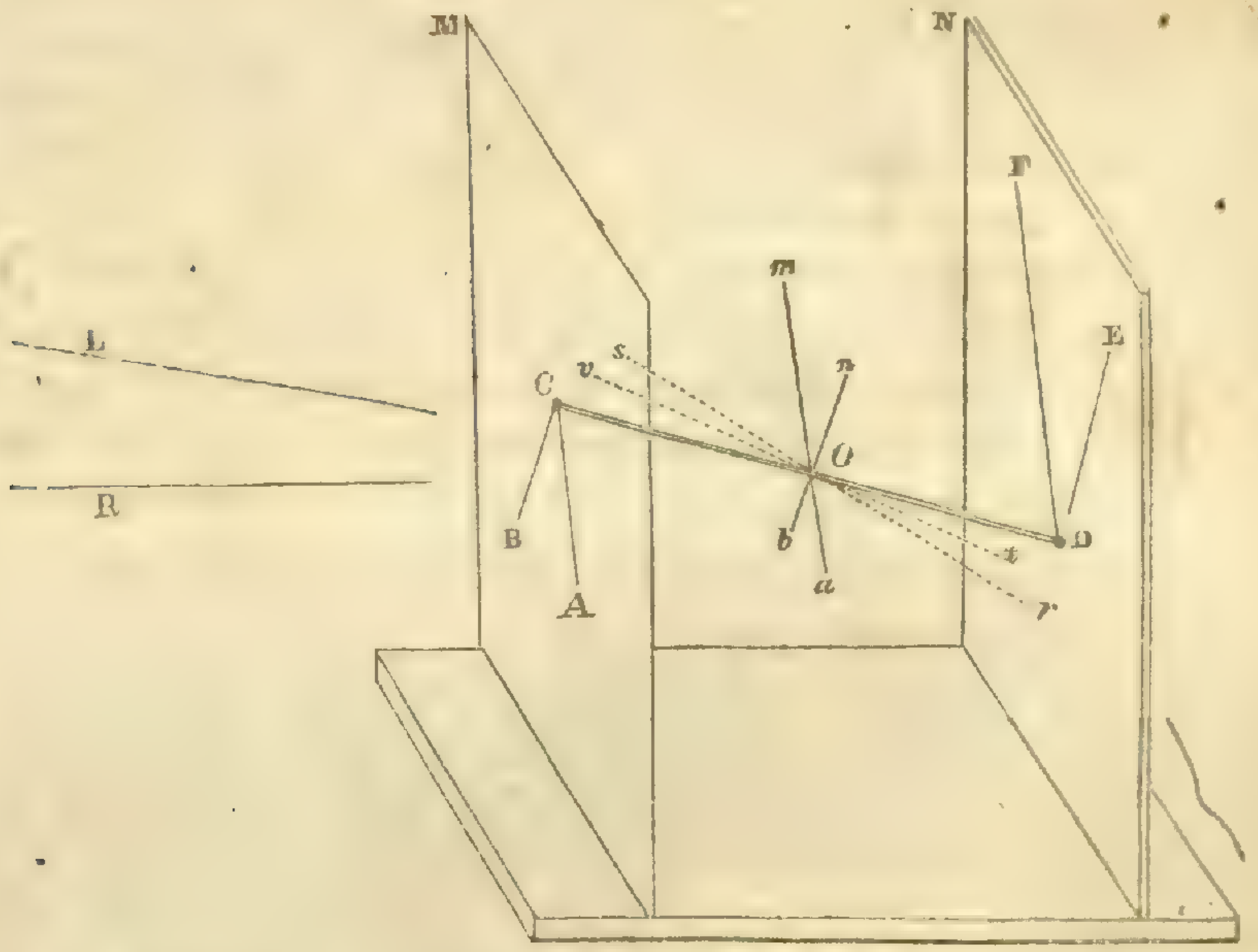
16. *Conditions of the binocular vision of a physical line in a perspective position.*

In describing under a former head (13) the appearances of a physical line or wire placed so as to coincide with the resultant of two inclined lines, particular mention was made of the perspectiveness of the two intersecting images of the wire as well as of the component lines of the resultant. A simple means of studying the former effect is obtained by fastening the point of a penknife into the end of a slender rod of wood (one of Faber's pencils) at right angles to its length, and holding the rod in a steeply sloping position between the eyes and a sheet of paper placed on the table. When the rod has been adjusted so as to be distinctly seen by each eye, as for example in the position CD (fig. 27), the eyes being at R and L, if we converge the axes *steadily* upon C or D or any intermediate point as O, we see the rod double, the two images appearing in a plane passing through C D or O at right angles to the direction of vision. Taking C for the point of axial convergence, the images will have the positions A′C and B′C; for D they will be represented by DE, DF, and for *o* by *am*, *bn*. But if we glance along the rod without making a continued pause at any point of its length, the two images will be seen to have a perspective position, continually varying in amount, at the same time that their mutual inclination and the point of their intersection changes. So long as the rod is held in this steep position in regard to the plane of the paper, the two images never approach so closely as to appear even for a moment to coalesce, and when we incline the rod at a much less angle, although this transient appearance of union often occurs, the general effect is that of two nearly coincident intersecting perspective lines.

Instrument for the binocular analysis of a perspective line.

The following simple apparatus (fig. 28), suggested by the above experiment shows in a very striking manner the variable double appearance of a physical line viewed in the conditions above described. It consists of two rectangular plates M and N, the former of clear glass, the latter of thin board covered with white paper and each about five inches wide by ten high. These

28.



are firmly fastened in a vertical position and parallel to one another, at an interval of 7 inches in a rectangular base of wood. The object used as a physical line is a thin uniform black cord one end of which is temporarily fastened to the inner side of the glass plate at some point C by a small shot-like lump of wax, while the other carried through a hole D in the vertical board is fastened by a similar piece applied behind D on the outer side of the board. Different inclinations are given to the cord by raising or lowering C, fixing it in the new position by the wax and then tightening the cord through the hole D.

It is needless to detail the various appearances of CD to the eyes supposed to be placed at RL, since they are the same as have just been described, with the advantage of being more clearly exhibited. I will merely say in explanation of the figure, that ACB is the aspect presented by the line when the eyes are steadily converged to C, EDF that when they are fixed on D, and *am bn* that when they are fixed on O; and that in the ordinary glancing vision from point to point of CD, it presents the appearance of the two slightly inclined perspective lines *rs tv*.

It can hardly be doubted that in thus viewing a physical line, the sense of perspectiveness associated with each of the two images has its origin in the successive direction of the optic axes to contiguous points, and as precisely the same appearances accompany the binocular union of two lines to form a perspective resultant, we are thus furnished with a confirmation of the theory before suggested that in this latter case the perspectiveness of the components originates from successive convergence to contiguous points along the imaginary resultant line. Thus while the stereoscope presents us the *binocular synthesis* of a perspective line

and the instrument just described gives us its *analysis*, they both lead to the same conclusion as regards the elementary impressions which are combined in the perception of such a line through binocular vision.

It is important to remark that in both these cases *when the component perspective lines are nearly coincident*, the perception to which they give rise in the ordinary rapid mode of vision is that *of a single resultant or a physical line, in a perspective position.* If we imagine such a line to be observed for the first time, there can be no doubt that it would appear confused and double until by looking at it with one eye only, or by turning it into a position transverse to the direction of view, and by applying the sense of touch from end to end, we rectify the impression made by the two nearly coincident intersecting images first perceived. By repetitions of this process, experience permanently connects the idea of a single perspective line with two nearly but not quite coincident intersecting images, of which the point of intersection continually changes its distance from the eyes. In a word, this *double image, peculiar in its form and variations, becomes the optical sign of a perspective line, and whenever impressed upon the eyes, it must by the law of association excite in us the idea of a single line in a perspective position.*

Thus in the case of binocular vision the perception of a perspective line *as single* is necessarily suggested by the two non-coincident pictures, provided their relation to one another is such as has usually accompanied our vision of such a single line. But when the two intersecting images make a considerable angle they depart from these conditions, and hence although as before explained, each appears in more or less of a perspective position, they do not by their united impression suggest the idea of a single perspective line. Thus when we view the cord CD in fig. 28 nearly endwise, we see two greatly divergent images producing a confused impression which, unless helped by information drawn from other sources, would leave us in doubt as to the true form of the object before us. The same uncertainty attends our judgment in regard to the perspective resultant in the stereoscope when its two lines intersect at a considerable angle.

17. *Of the extent to which the successive combination must be carried.*

From what precedes it would appear that the perception of perspectiveness in a binocular resultant or in a physical line requires a successive direction of the axes to contiguous points along the line. To what extent such successive vision must be carried in order to create a definite perception of perspectiveness is a question of much interest and difficulty.

According to the view of Sir David Brewster no part of the object is seen single and distinctly but that to which the optic

axes are for the moment directed and "the unity of the perception is obtained by the rapid survey which the eye takes of every part of the object," (Phil. Mag., May and June, 1844). On this theory the conditions of binocular vision of a perspective line would be as follows:

1. The perception of a perspective resultant line in the stereoscope would require the optic axes to be successively directed so as to unite every pair of corresponding points of the two component lines of the diagram, or which amounts to the same thing, they should be successively converged to every point of the perspective resultant.

2. This resultant would of necessity be perceived as a single perspective line the locus of all the resultant points.

3. In the case of two intersecting lines appearing instead of this resultant, these lines should neither of them have a perspective position.

But these conditions differ in important features from the phenomena of the perspective resultant before described. In the first place the perspectiveness of the resultant, although not perceived when the axes are steadily fixed at any one convergence, appears as soon as *the varying convergence has united contiguous points* of the component lines, and it affects the whole extent of the resultant, although the observer may have restricted the successive union to only a small fraction of the length of those lines. In the second place the perceived resultant instead of being a single line, is in most cases composed of two intersecting perspective lines varying their point of intersection and their degree of relief. The same conditions, as we have seen, apply to a physical line held in the position of the binocular resultant.

It would then appear that the perception of a perspective resultant line or a physical line in the same altitude, does not require the successive convergence of the axes to every point. The direction and amount of the relief may be sufficiently suggested by the view of a few contiguous points. The presence of two apparently or nearly coincident images would not of itself convey the idea of one or of two *perspective lines*, but if, along with this, we have even through a short distance the impression of an increasing or diminishing convergence and a change in the united direction of the axes, the optical signs of perspectiveness may be regarded as sufficiently complete to give us the perception either of two intersecting perspective lines of variable relief or that of a single line with the maximum relief.

When the line is of considerable length, we assure ourselves of its straightness and the degree of its relief by directing the eyes successively to all parts of it, but when it is short the images in the two eyes are distinct enough throughout to make a perception of the whole line definite and clear, without more than a very

partial survey, and this seems to be all that we employ in the ordinary vision either of a physical line or a binocular resultant in a perspective position.

From what has now been said, it is apparent that the process by which we see objects in relief by the binocular combination of drawings, is *nearly* identical with that by which we see the objects themselves, but I cannot agree with Sir David Brewster that "it is the *very same* process as in the vision of real objects in relief by the ordinary agency of our two eyes." It is true that as regards the successive change of convergency of the axes the action of the eyes is precisely the same whether they are directed to different parts of the binocular resultant or of the real object which it represents; but as formerly remarked it does not appear that the change of convergency is accompanied in the former case by any alteration of refractive power, while in the latter case we know that every increase or diminution of the one is associated with a corresponding change of the other.

18. *Of binocular direction.*

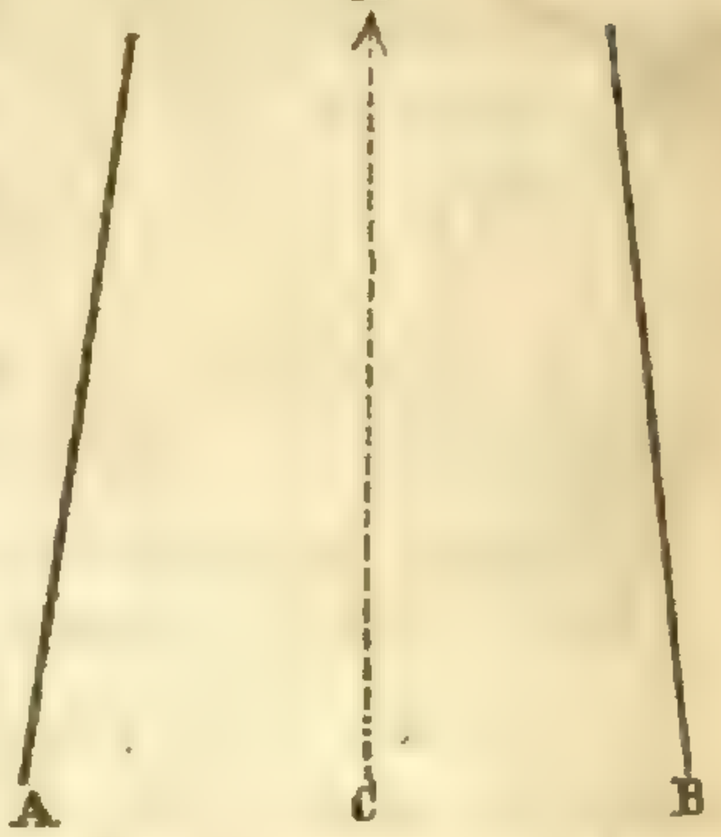
If on either stage of the stereoscope we place a diagram consisting of two dots or of two equal parallel lines, and after directing the eyes upon them so as to see them where and as they are, we then by the usual effort cause them to unite into one, we perceive that in their approach they move towards an intermediate position, and that the resultant dot or line lies in a direction midway between those in which the eyes previously saw the two separate objects. The peculiar sense of direction of which we are conscious in this and similar cases is evidently a resultant impression, derived from the co-existence or union of two different feelings of direction furnished by the two eyes. It may therefore I think, be not inappropriately called the *binocular direction.*

That in our ordinary vision with two eyes we have a similar sense of the binocular direction of objects is I think, proved by a variety of familiar facts. Thus for example, in aiming to strike an object with a stone or javelin, or in shooting at a mark with a bow or a gun held up to the breast, we endeavor to conform the motion which we impress to the feeling of direction derived from the combined use of the two eyes, and in the game of bowls it is this which guides the hand from which the ball is discharged.

The importance of this peculiar sense of direction, as distinguished from that of the eyes used singly, becomes more striking when we consider it in connexion with the perspective resultant formed by binocular combination, as in the following examples: In each case the combination is supposed to be made on the upper stage of the stereoscope, and, for the sake of clearness, the resultant is represented by a dotted line, the arrow head marking the nearer or upper extremity.

In viewing the perspective resultant CD of the lines A and B (fig. 29), each of these lines seems to occupy the position of the resultant, that is, each appears to lie in a direction midway between the two directions in which the components are seen without binocular combination. So when the resultant has the form of two intersecting nearly united perspective lines, each of them appears in a position nearly midway between those of the components. In this experiment the place of the resultant, that is the binocular place of both A and B, is in the vertical mesial plane of view, while that of A seen by the left eye singly is an oblique plane diverging to the left, and that of B seen by the right eye singly is in a similar plane diverging to the right.

29.



By covering or omitting a part of one of the components as in fig. 30, and combining them by the usual binocular effort, we have a resultant consisting of a perspective line ED due to the union of A and the corresponding part of D, and a line EC not perspective but parallel to B and equal to the residuary part of it. Here by the binocular combination the upper half of B is made to appear in a direction different from the lower half not thus combined and therefore different from the direction in which it would itself appear independently of the combination.

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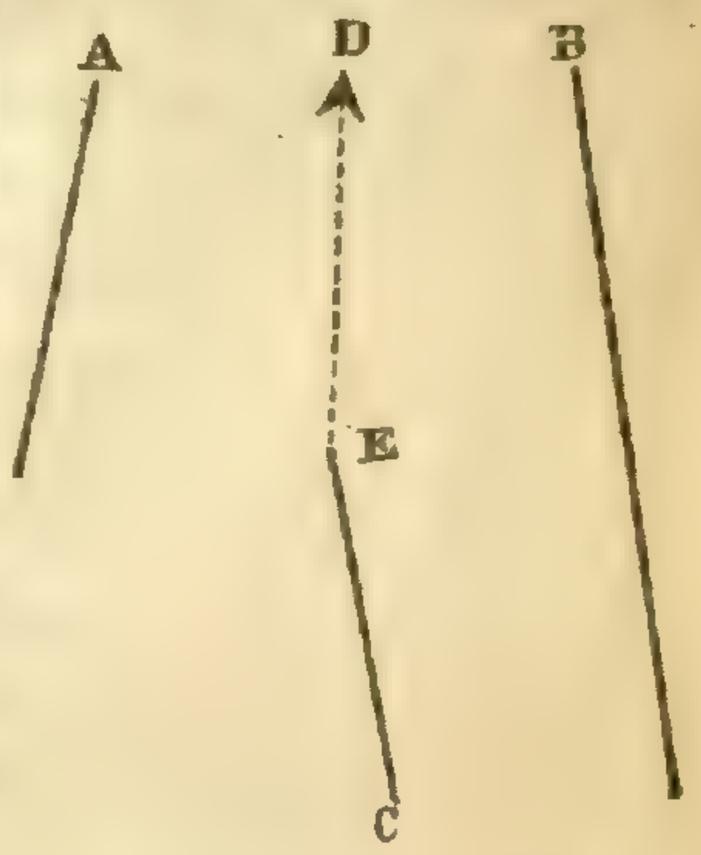

That the same influence operates in ordinary vision with both eyes is shewn by the following experiment. (Fig. 31.) Let

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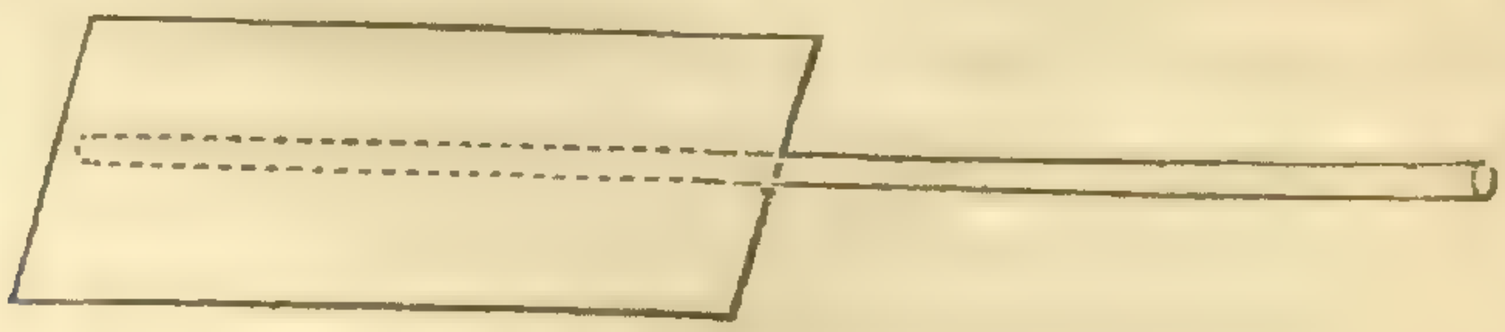


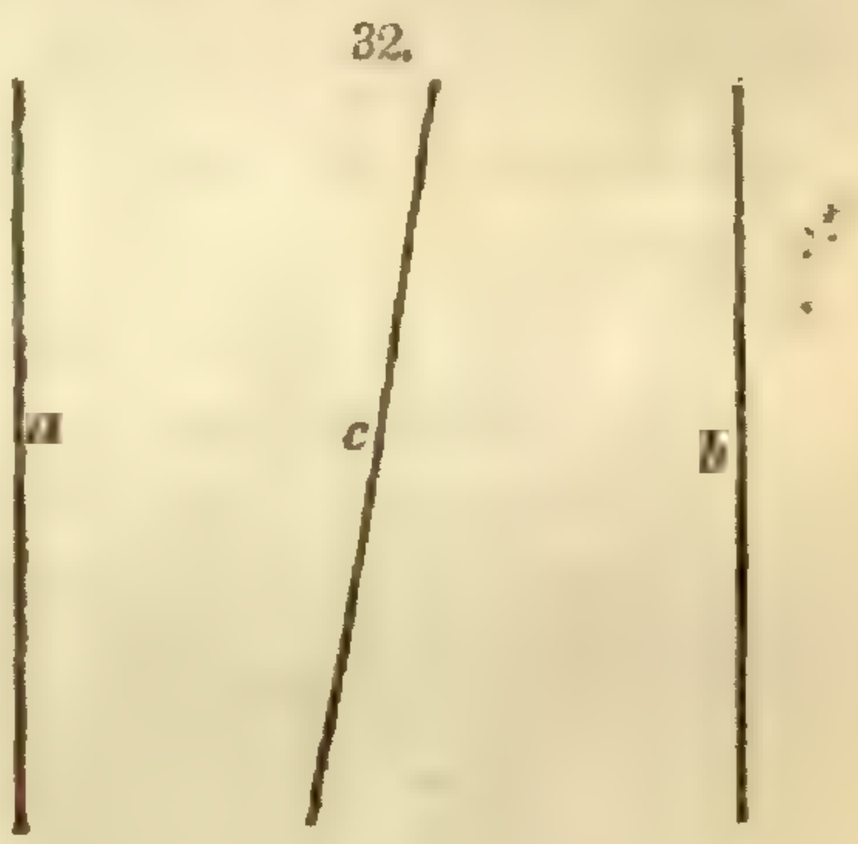
a line or thin rod be held at the limit of distinct vision in a perspective position between the two eyes, while by a card attached laterally we hide the near half of the rod from the right eye. If now we direct both eyes upon the rod, it will appear not as a straight line but as composed of two parts making an angle in the middle, the remote half lying in the mesial direction of our view, and the near half deflected a little to the right. The re-

sult of this simple experiment would be inferred from what has been said under a previous head, but it is so striking as at first to create some surprise.

Since the binocular direction of an object is not that in which it appears to either eye separately, it follows that two lines having similar attitudes to the two eyes respectively, may be made to appear in very dissimilar positions by combining a third line with one of them and not with the other.

Thus if we place fig. 32 on the upper stage so that *a* shall be opposite the left and *b* and *c* opposite the right eye, and if then we combine *c* with *a*, we shall have a perspective resultant (fig. 33) lying obliquely on the left of *b*. Now this oblique resultant is the binocular place of *a* as well as of *c*, and thus the binocular combination has caused *a* which is really parallel to *b* and like *b* in a vertical visual plane to appear in an oblique visual plane receding from that of *b* downwards towards the left.

32.



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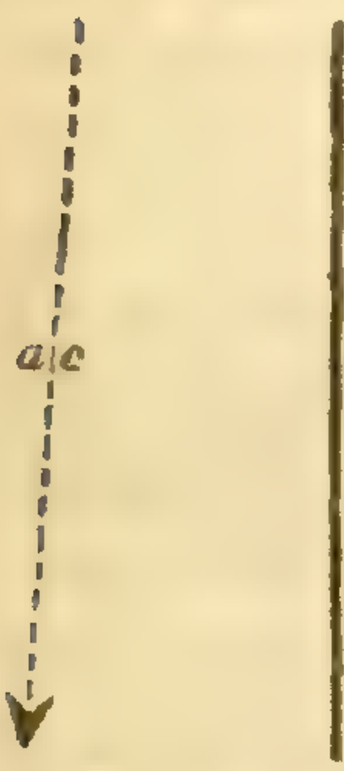

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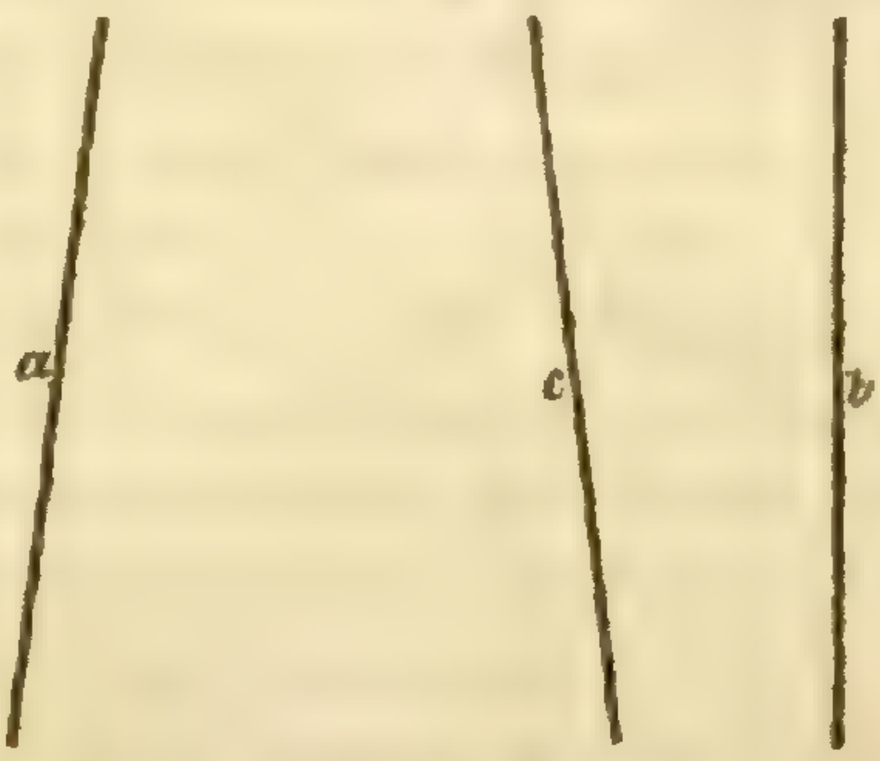

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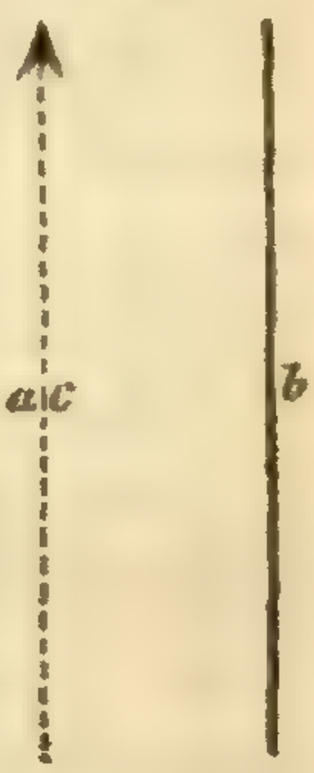
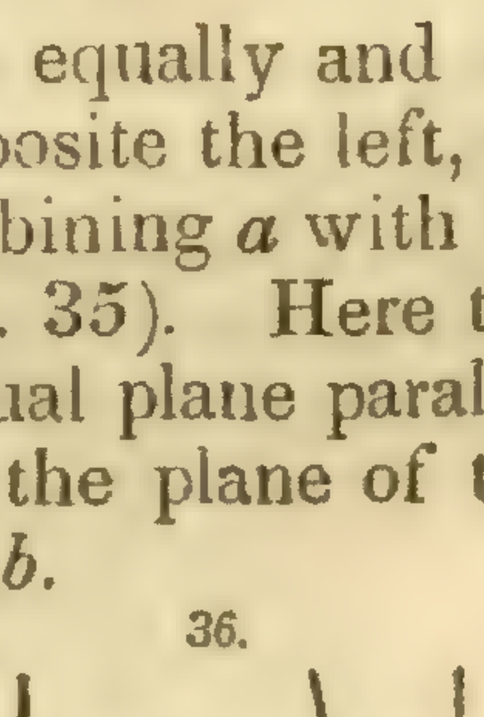


Using fig. 34 in which *a* and *c* are equally and oppositely inclined to the vertical, and placing *a* opposite the left, and *b* and *c* opposite the right eye, we have, on combining *a* with *c*, a resultant lying in the mesial visual plane, (fig. 35). Here the binocular position of *a* and *c* is in a vertical visual plane parallel to that of *b*, and when projected binocularly on the plane of the paper, will appear to have a position parallel to *b*.

With lines related as in fig. 36, the effect is still more interesting. Here *a* and *c* when united form a perspective line which springs from the plane of the paper at the lower end of *b* and ascends obliquely towards the left (fig. 37). If in effecting the combination we look towards the lower ends of the lines where *b* and *c* are equidistant from *a*, a pause in the convergence will unite *a*

36.



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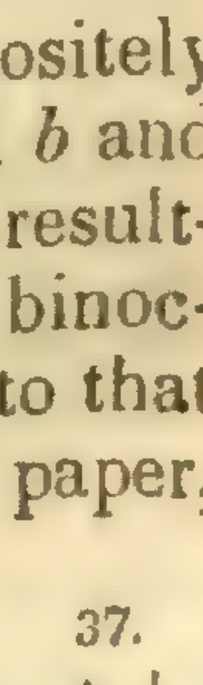


and *b* while *c* appears in the same plane and at the same inclination as in the diagram. But the moment the convergence increases, *a* and *c* unite and the perspective resultant rises from the plane of the paper.

Now in effecting the binocular union while looking towards the lower end of the lines, it is evident that for a moment *a* and *b* both form their images centrally on the retinal surface of the two eyes, and therefore according to the received law of single vision by corresponding points they might be expected to appear as a single line, that is to say, in the same place. That this coincidence does not occur is explained by the fact that *a* is seen in the binocular direction and that this direction being compounded of the impression made by *a* and *c* cannot be the same as that due to *b* alone or to *a* and *b* combined. Referring more directly to the visual actions concerned, we would say that in virtue of changing convergence the perspective idea becomes in this case associated with *a* and *c*, but no such idea is attached to *b*. Hence even when the images of *a* and *b* fall on corresponding points of the retinas, the conditions under which they are perceived are different and the two lines should not be expected to appear in the same place and direction.

The fact here demonstrated, that the images formed on corresponding points of the eyes may yet be perceived in separate places, is certainly at variance with the law of corresponding points, as usually expressed. *The contradiction however disappears, when, as is obviously proper, we restrict the application of the law to the cases in which the images are neither of them disturbed by combination with a third image.*

Another feature of the experiment worthy of especial notice is the fact that although the images of *a* and *b* are always formed on the retinas in parallel positions, no matter to what point of the perspective resultant we may for the moment direct our eyes, yet we never perceive them as parallel. The explanation is obvious. We perceive *a* in the binocular direction, that is, in the position of the resultant formed by the union of *a* and *c*, and therefore in an oblique plane, while we see *b* only in its natural vertical position.

19. *Experiment of Prof. Wheatstone to disprove the theory of corresponding points.*

Prof. Wheatstone adduces the following experiment to show "that similar pictures falling on corresponding points of the two retinæ may appear double and in different places."

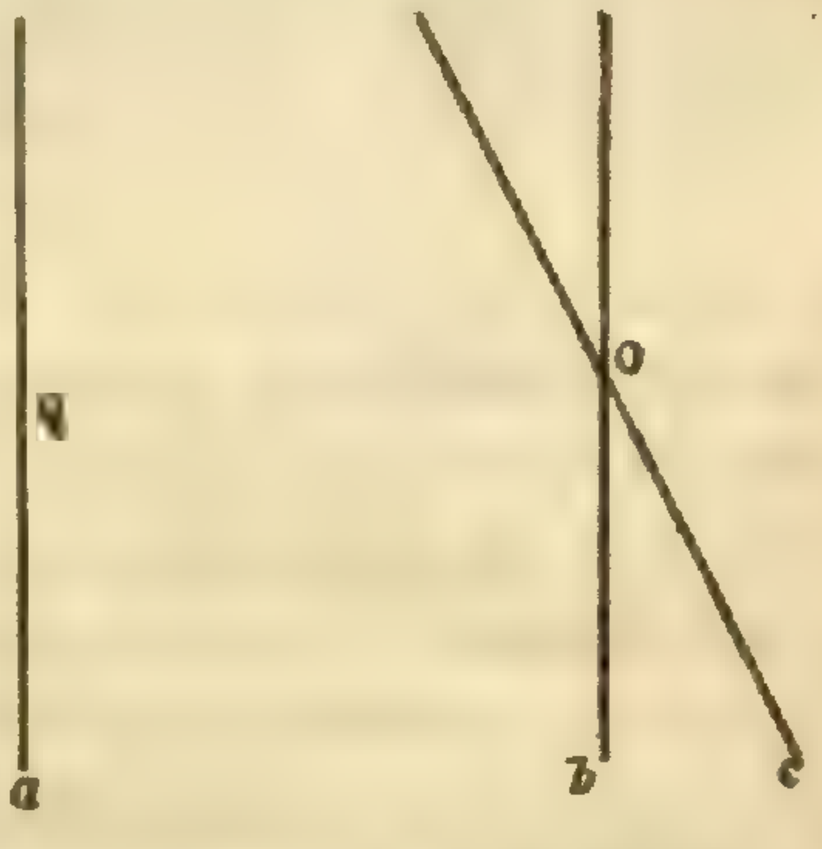
"Present in the stereoscope to the right eye a vertical line and to the left eye a line inclined some degrees from the perpendicular; the observer will then perceive, as formerly explained, a line the extremities of which appear at different distances before the eyes. Draw on the left hand figure a faint vertical line exactly corres-

ponding in position and length to that presented to the right eye, and let the two lines of this left hand figure intersect each other at their centres. Looking now at these two drawings in the stereoscope the two strong lines each seen by a different eye will coincide and the resultant perspective line will appear to occupy the same place as before; but the faint line which now falls on a line of the left retina which corresponds with the line of the right retina on which one of the coinciding strong lines, viz., the vertical one, falls, appears in a different place." The author adds, "This experiment affords another proof that there is no necessary physiological connection between the corresponding points of the two retinas,"* &c.

In this experiment the visual action and the consequent perception are analogous to those above described, and the explanation of the result is in all respects the same. As however too general a conclusion, has I think been drawn from it by the ingenious author, and as it has been the subject of an entirely different interpretation by Sir D. Brewster, I propose to consider it more in detail.

Using a drawing of the proportions represented in fig. 38, and placing it on the upper stage of the stereoscope so that *a* may be opposite the left and *b* opposite the right eye, it is easy by a quick change of convergence to combine *a* with *c*. The perspective resultant thus formed is seen intersecting the plane of the diagram and the line *b* in the point O. If we carefully graduate the convergence so as to mark the different stages of the phenomenon we may bring *a* to touch the upper end of *c* then to cross it successively nearer the middle, then to coincide with *b* then to cross on the other side of the centre, and lastly to touch the lower end of *c*. When this is done slowly the line *a* is seen in the various positions represented in fig. 39, always parallel to *b*.

38.



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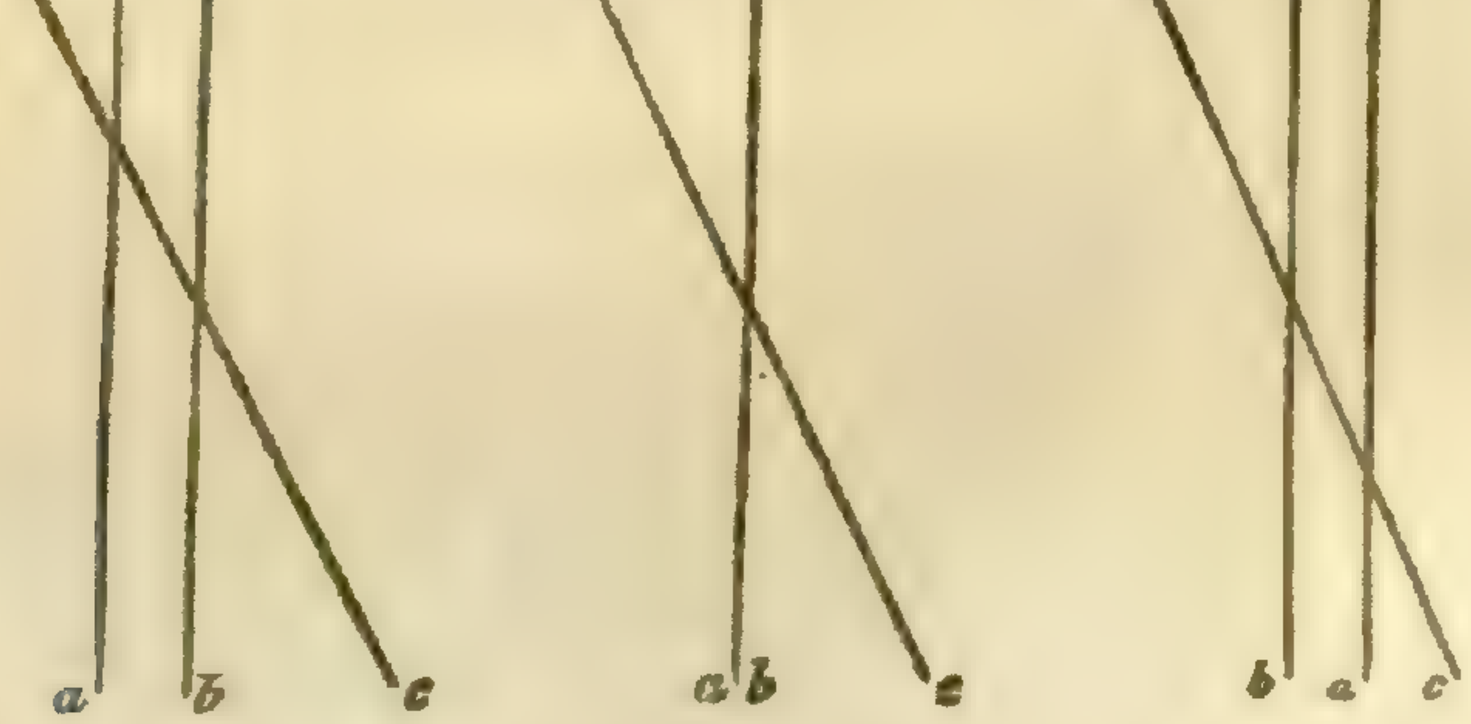

* Phil. Trans., 1838, Part 2, or Phil. Mag., April, 1852.

If however we produce the intersections in somewhat quick succession, we have a perception more or less distinct of the perspectiveness of each of these intersecting lines, while *a* and *c* tending to coalesce causes the former to lose its parallelism to *b*. When the perspective effect is fully developed we perceive *a* and *c* only as they are united in the resultant. The axial movement has connected the sense of perspectiveness with each of them causing each to be perceived in the *binocular direction*.

Now it must be remembered that this perspective position and binocular direction being the result of successive combination of points in the two lines, is produced as well when the contiguous points thus united lie near the centre as when near the ends of the lines. But in the former case, that is, where the points about O in *c* are united with those about O in *a*, the line *b* must form its image centrally in the right eye when *a* forms its image in like position in the left. In these circumstances therefore the retinal images of *a* and *b* fall on corresponding points, at the same time that the former line is perceived in the binocular perspective position intersecting but not coinciding with the latter. In this momentary phasis of the experiment, it is obvious that the result is at variance with the law of corresponding points. When however the parts of *a* and *c* which the eyes successively combine are not near the centres of these lines, the image of *b* will be formed *away from the retinal centre* of the right eye, while that of the combining points of *c* will be in that centre, and the image of ***a will be in the retinal centre*** of the left eye. In this case the law of corresponding points requires that *a* and *b* should appear distinct from one another as they really do. Thus therefore ***it is only when the resultant perspective is formed by combining the central points of the component lines that this experiment contradicts the law in question.***

But the characteristic feature in this class of effects is not so much the departure in certain conditions from the theory of corresponding points, as the general fact of which this is but a particular instance, viz., ***that each of the lines binocularly combined is by virtue of the combination perceived in a position different from that in which it is seen without such combination, although in both cases the image of the line has the same retinal position in the same eye.***

In explaining the experiment of Wheatsone, above referred to, Sir David Brewster, if I rightly understand his remarks, (Phil. Mag., June, 1844,) ascribes the disappearance of *a* as a vertical line to an actual invisibility of the vertical image, or part of it, caused by its proximity on the retina to the image of the other component line. Of the fitful vanishing and reappearing of parts of the figure long since noticed in such cases by this eminent observer, I am abundantly satisfied from my own experience; but

I cannot see in the phenomenon, even were it of constant occurrence, an explanation of the appearances in question. Such a view seems to me inconsistent with the fact always observable in these combinations when not too rapidly made, that the vertical and other component lines assume a perspective attitude while still intersecting one another at an appreciable angle, that in this position the former has already lost its appearance of parallelism to the other vertical, that the two intersecting perspective lines in spite of an occasional disappearance in part, continue each generally visible until they have so closed upon one another as to appear coincident, and that even when thus united *the presence of both of them in the resultant is shewn by the continued visibility of the dots or other marks intended to distinguish them.*

In this experiment, various special phases of combination between the lines *a* and *c* present themselves, according to the direction and range of the axial movement by which the vision is effected. Sometimes the upper halves only, and sometimes the lower halves of the lines, are united, presenting an effect identical with that of fig. 36, previously described. But these and other features of the experiment being unessential to the present inquiry, need not be considered in this place. They will be treated of hereafter in connexion with a variety of analogous phenomena, belonging to the class of alternating or changeable combinations.

20. *Combination of lines lying in the same horizontal direction.*

When two *equal* right lines, fig. 40, lying in the same horizon-

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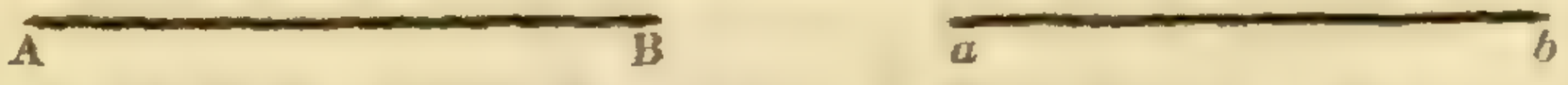

tal direction at right angles to the line of view are united binocularly either behind or in front of their real position they form, as might be expected, a resultant parallel to the components. The same convergence which serves to combine any two corresponding extremities as A and *a* causes also a union of the other ends B and *b* and combines all the parts of corresponding points of the two lines. When therefore the lines are so short as to be distinctly visible throughout their length without a change of axial direction, the formation of the resultant behind or before the components and parallel to them must be regarded as simultaneous throughout.

In the case of *unequal horizontal lines* the results are very different. Here the combination is to a certain extent successive involving a change of convergency and giving rise to a perspective resultant.

Sir David Brewster, in discussing the question of the coincidence of unequal figures adduces the first observatiou on this sub-

ject of which I have seen any description. "The best way," he says, "to make this experiment is to take two lines AB *ab*, (fig. 41,) of unequal lengths, and with a large pin to perforate

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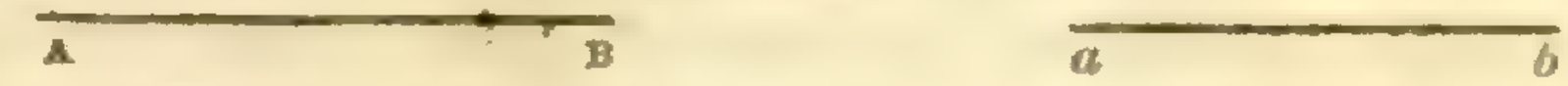


the lines at AB *ab* so that when we attempt to unite them we shall see with perfect distinctness their four luminous extremities. When the point *a* is made to pass into A, I have never succeeded in making *b* pass into B. Whenever there is an appearance of this either turn round the paper or the head so as to separate the lines and it will be invariably seen that if *a* springs out of A, *b* will spring out of a point between A and B. The apparent coincidence therefore of AB with *ab* when it is seen arises from a *disappearance* of one or other of the extremities of the two lines."

In repeating this experiment in various forms, I have observed the effects described by Sir D. Brewster, but in addition to these I have obtained other results, which furnish a different and I think more satisfactory explanation of the optical coincidence of the unequal lines. Drawing the two lines in very opaque ink on somewhat thin letter paper, of the lengths of one inch and eight-tenths of an inch respectively, with perforations at the ends, I place the paper at the distance of distinct vision between the eyes and a bright window or the globe of a solar lamp, and by the usual binocular effort bring the two lines together—either behind or in front of their true place.

At first, if the axial movement be slow, I see the two luminous ends of the shorter line vibrating on the longer one, and if I bring *a* and A to coincide I see *b* and B separated, or making the latter coincide I see *a* and A separated, and all this time the two lines appear to lie in the plane of the diagram. But carrying the eyes from one end of the resultant to the other and uniting the the two ends in succession by a quick change of convergence, I observe the resultant suddenly assume *a perspective position.*

In my own case, this effect usually presents itself almost immediately, but persons unpractised in such experiments perceive at first only a confused impression, which, after they become accustomed to the requisite axial movements, gives place to a clear perception of the perspective line. The direction of the relief is of course dependent on the place at which the binocular union is effected as compared with the actual distance of the diagram. When the eyes are converged beyond the paper, the extremities *b* and B are united at a point nearer the observer than are *a* and A, and therefore the resultant recedes towards the left. When the axes meet in front of the paper, *b* and B are united at a greater distance than *a* and A, and hence the resultant recedes towards the right.

In this experiment the resultant appears as nearly single as in the case of two slightly inclined lines (p. 212) so that unless we pause to fix the gaze on one of the ends we cannot observe a want of coincidence at the other extremity. The moment however we make such a pause, the two luminous points of the other end separate, that of the short line moving on the long one to a distance equal to the difference of their lengths, and at the same time the resultant subsides from its perspective position.

When the coincidence of the two lines is such as to present the perspective effect, I cannot observe the disappearance of one or other extremity as mentioned by Sir D. Brewster. As a test of this I place a small dot on the outer edge of the perforations at A and *b* and I find that these marks always remain visible while the resultant is seen in perspective. I may add that such a disappearance would not explain the seeming coincidence of the unequal lines in a *perspective* resultant, but that on the contrary the formation of this resultant, as in the case of mutually inclined lines requires the actual though not simultaneous *union of the corresponding ends.*

The effects above described are readily obtained by using lines marked at the ends by short cross lines or by dots placed below or above them, and effecting the combination either without a stereoscope or by placing the figure on the upper or lower stage.

42.

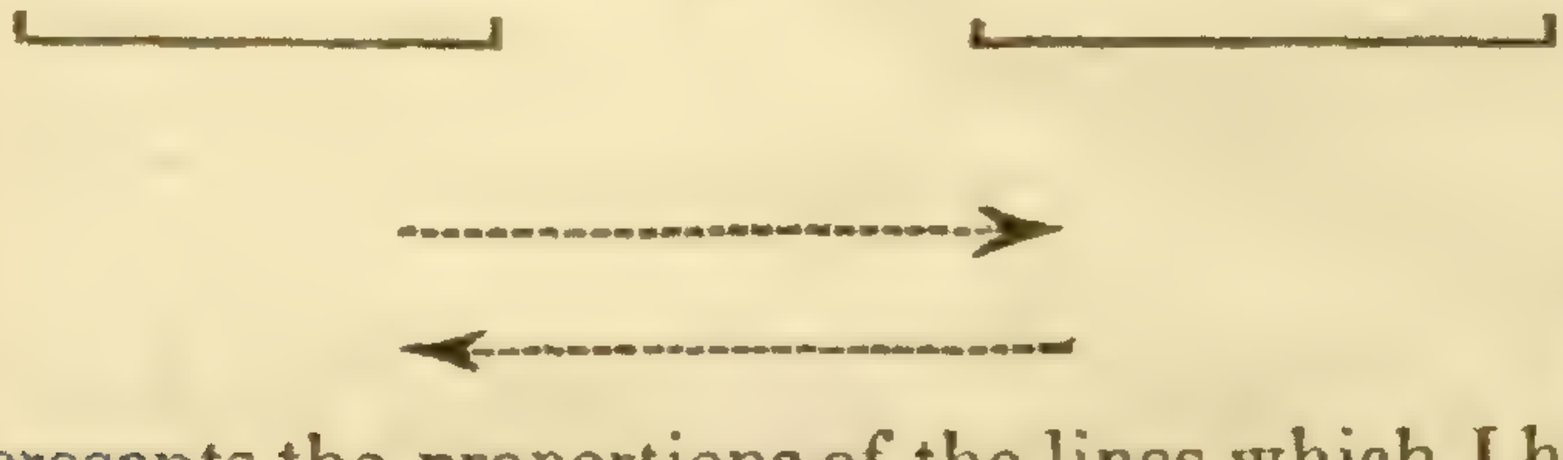
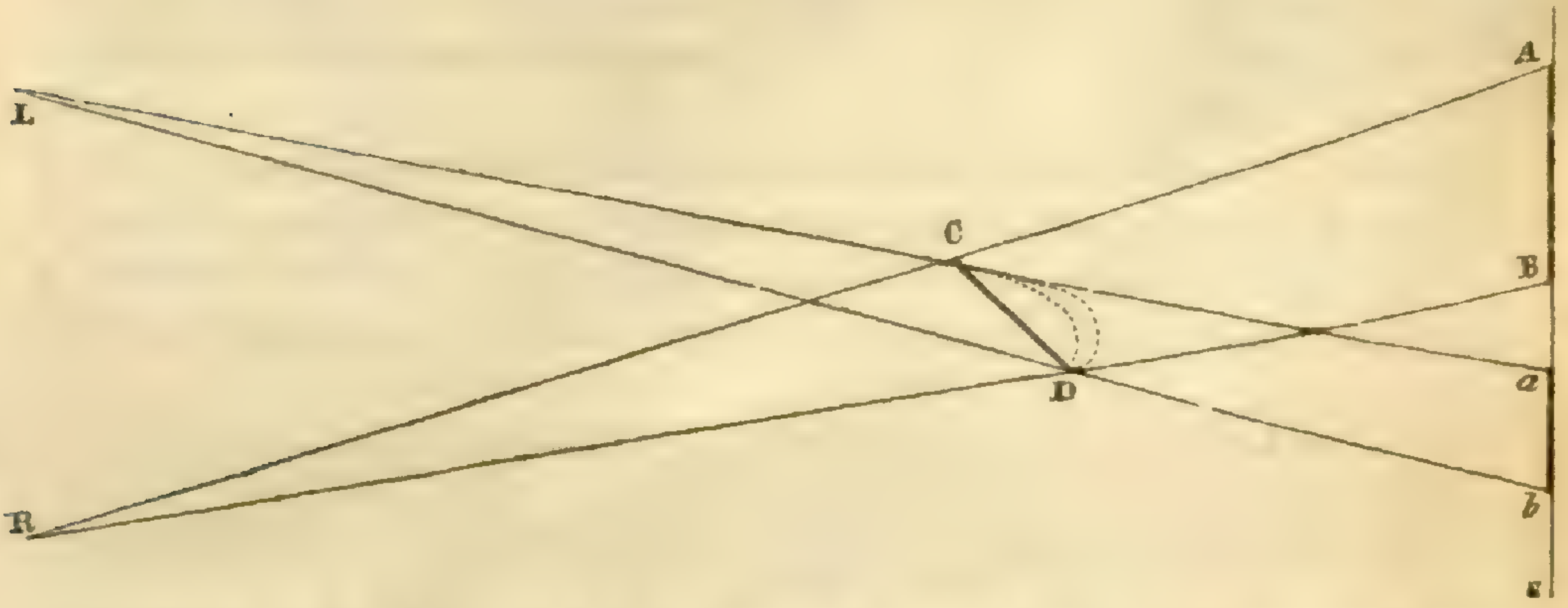


Fig. 42 represents the proportions of the lines which I have found convenient for either of these methods of operating. With this I obtain at once or very promptly, the binocular coincidence and the perspective effect. The dotted lines below denote the perspective directions, the arrow heads marking the nearer ends of the resultants. The upper of these indicates the resultant when the lines are united by using the upper stage or by looking beyond the paper, the lower when they are united by the lower stage, or at a plane nearer than the paper.

When the lines are not, as in the preceding experiments, strongly marked at the ends, the effect of the combination is variable, presenting sometimes a partial superposition of one line or the other without relief and sometimes a perspective resultant; I find however that by using broad lines strongly contrasting with the paper, I can in general obtain the latter result quite promptly, but the effect is more fluctuating and difficult to maintain than when the ends are conspicuously marked.

It is to observed that in all these experiments the perspective effect is directly dependant on the successive union of the cor-

responding *ends* of the lines, and that the two resultant points or short vertical lines thus formed are produced in the same way as the two resultants of unequally distant verticals treated of under a preceding head. The intervening points of the horizontal lines being all alike are not, as in the case of the extremities, marked out in corresponding pairs, and therefore may be combined binocularly in various ways, according to the rate of variation of the axial convergence in the interval between the terminal combinations. On a close examination of the resultant, I find accordingly, that while the actual amount of its relief remains constant, the intermediate parts of the line do not always appear in a straight direction between the near and remote end. Thus supposing the combination to be made in front of the paper MN (fig. 43) and that C and D represent the points at which the corres-

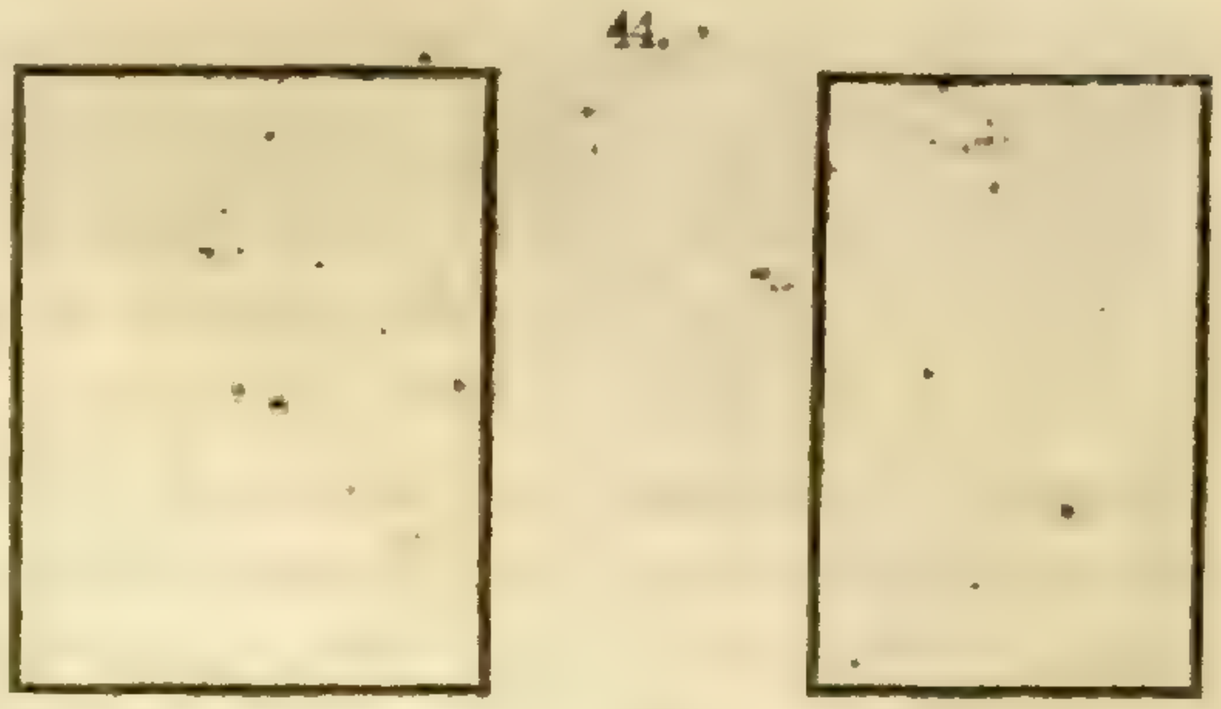


ponding ends A *a* and B *b* appear combined, I observe sometimes that the resultant loses the clear and satisfactory character of the straight perspective line CD, and appears vaguely as a curve or broken line retreating from the former towards the plane of the paper, but always terminating in C and D. The usual and more permanent form CD is obviously due to a uniform change of convergence in passing from C to D; the other more vague and evanescent appearances are I think produced by variable converging movements in the same interval.

From these experiments, we are I think entitled to conclude that *unequal horizontal lines not greatly differing in length may be combined so as to form a perspective resultant, the right and left ends of which are at unequal distances from the observer.*

This result, which appears to have escaped the attention of preceding experimenters, is of importance from the light thrown by it on the binocular union of unequal figures. In this connexion it will be referred to under a future head, but in the mean time the following example of its application to this question may properly conclude the present division of our subject.

44.



On combining the two rectangles (fig. 44), either without a stereoscope or by using the upper or lower stage of the sliding instrument, I find that the resulting figure always presents itself *in a perspective position*, of which the near vertical edge, as might be expected, appears shorter than the remote one. When the most perfect and steady effect is obtained the upper and lower edges appear as straight lines and the whole perspective figure seeming to be a plane. But sometimes in beginning the observation and occasionally when the eyes have grown fatigued, these lines assume the concave direction mentioned above, and the resultant figure has the aspect of a bent or warped surface. I may add that the perspective effect is clearly seen when the same figures are combined by means of one of Wheatstone's or Brewster's stereoscopes.

NOTE.—In the preceding No. on page 216, the right-hand figure of the twin diagram 25, should have been reversed as in 26.

(*To be continued.*)

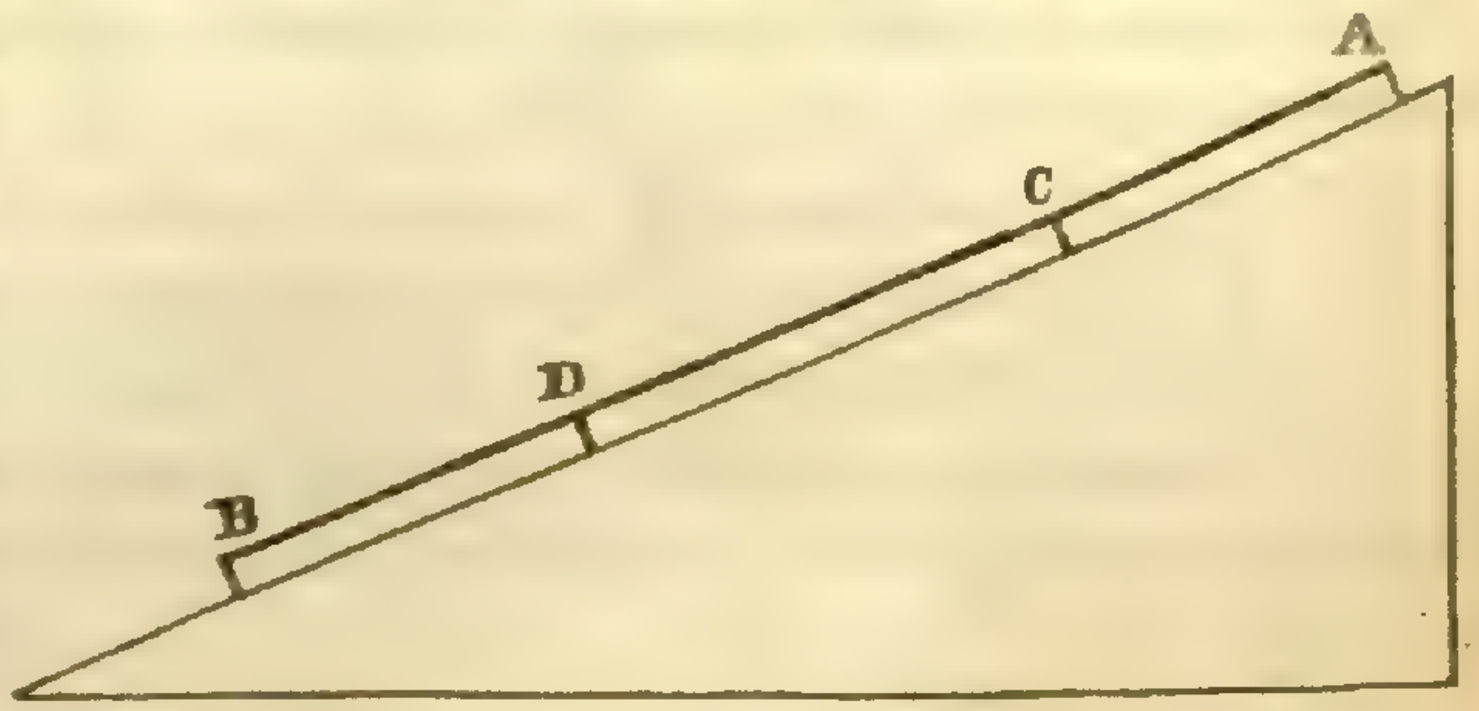
ART. XXIX.—*Remarks on the Rev. Henry Moseley's Paper, "On the Descent of Glaciers;"* by JOHN LECONTE, M.D., Professor of Nat. Philos. and Chem. in the Univ. of Georgia.

THE Abstract of the Proceedings of the Royal Society for April the 19th 1855, published in the "Philosophical Magazine" for July, 1855, (4th Series, vol. x, p. 60 et seq.,) contains a very ingenious communication from the Rev. Henry Moseley. In this paper it is shown, by the application of established mechanical principles, that a solid body resting upon an inclined plane, whose angle of inclination is *less* than the limiting angle of resistance, will gradually *descend* by means of the *elongations and contractions incident to the ordinary changes of temperature.* To render this intelligible to those who may not have access to the paper of Mr. Moseley, I add the following condensed account of his investigation.

If we conceive two equal bodies (cubes for instance) of the same material, connected by a metallic rod, to be placed on an inclined plane (one being lower than the other), then when, by the increased temperature of the intervening rod, its tendency to extend becomes sufficient to push the *lower* of the two bodies *downwards*, it will not have become sufficient to push the *higher upwards*. The effect of its extension will, therefore, be to cause the lower of the two bodies to descend whilst the higher remains at rest. The converse of this will result from contraction: for

when the contractile force becomes sufficient to pull the upper body down the plane, it will not have become sufficient to pull the lower up it. Thus, in the contraction of the intervening rod, the lower body will remain at rest whilst the upper descends. It follows, therefore, that if the connecting rod is heated and then allowed to resume its former temperature, both of the bodies will descend the plane by an amount equal to the whole linear elongation of the rod. This will be repeated as often as there are oscillations of temperature, until step by step, they reach the bottom.

Next, suppose the uniform bar AB placed on an inclined plane, and subject to extension from increase of temperature ;——a portion CB will descend, and the rest CA will ascend ; the point C where they separate being determined by the condition, that the force requisite to push CA up the plane is equal to that required to push CB down it. When contraction takes place, the converse of the above will be true. The separating point D will be such, that the force requisite to pull DB up the plane is equal to that required to pull DA down it. DB is obviously, in this case, equal to CA in the other. Under these conditions, the determination of the lengths of the parts CA=DB and CB=DA, becomes a simple mechanical problem, from which Mr. Moseley deduces the formula,



$$x=\tfrac{1}{2}\mathrm{L}\left(1-\frac{\tan i}{\tan a}\right),\ (1),\ \text{in which,}$$

x=CA=DB
L=length of rod AB
i=angle of inclination of plane
a=limiting angle of resistance.

Let c be the elongation per linear unit under any *given* increase of temperature ; then the distance which the point B will be made to *descend* by this expansion,

$$=c(\mathrm{CB})$$
$$=c(\mathrm{L}-x)$$

By (1) $$=\tfrac{1}{2}\mathrm{L}c\left(1+\frac{\tan i}{\tan a}\right)$$

If we conceive the bar now to return to its former temperature, contracting by the same amount (c) per linear unit ; then the point B will by this contraction be made to *ascend* through the space

$$=c(\mathrm{DB})=cx$$
$$\tfrac{1}{2}=\mathrm{L}c\left(1-\frac{\tan i}{\tan a}\right)$$

The *total* descent (N) of the point B, by the elongation and contraction, being obviously equal to the *excess* of its *descent* above its *ascent*, is, therefore, determined by the equation

$$d=\tfrac{1}{2}\mathrm{L}c\left(1+\frac{\tan i}{\tan a}\right)-\tfrac{1}{2}\mathrm{L}c\left(1-\frac{\tan i}{\tan a}\right)$$

$$\therefore d=\mathrm{L}c\,.\,\frac{\tan i}{\tan a}. \quad (2)$$

His attention having been first drawn to the influence of variations in temperature to cause the descent of a sheet of metal resting on an inclined plane, by observing that a portion of the lead which covers the south side of the choir of the Bristol Cathedral, which had not been properly fastened, had descended bodily eighteen inches into the gutter; Mr. Moseley proceeds to show, by the application of the foregoing formula (2), that the cause assigned is adequate to account for the fact observed. Thus far Mr. Moseley's reasoning is perfectly satisfactory aud in accordance with what might be expected from one of the clearest and best writers on mechanical philosophy: but when he attempts to apply these facts and principles to the solution of the mechanical problem of the motion of glaciers, the *physical aspect* of his reasoning is singularly exceptionable. It is to this portion of the paper, that the attention of the reader is particularly directed.

After referring to the recent experiments of MM. Schumacher, Pohrt and Moritz, which show that the linear expansion of ice for a given increment of temperature, is greater than that of any other solid body hitherto examined; Mr. Moseley remarks, "Ice, therefore, has nearly twice the expansibility of lead, so that a sheet of ice would, under similar circumstances, have descended a plane similarly inclined, twice the distance that the sheet of lead, referred to in the preceding article, descended. Glaciers are, on an increased scale, sheets of ice placed upon the slopes of mountains, and subjected to atmospheric variations of temperature throughout their masses, by variations in the quantity and temperature of the water, which, flowing from the surface, everywhere percolates them. That they must, from this cause, descend into the valleys, is therefore, certain." (p. 64.) To show the adequacy of this cause, he applies the formula (2) to the Glacier de Léchaud, the Glacier du Géant, and the Mer de Glace. From the data in relation to length and inclination, furnished by the measurements of Prof. J. D. Forbes and others, assuming the mean *daily* variation of temperature to be 5°·321 Centigrade,* and the linear expansion of ice for an interval of 100° Centigrade to be

* This is the mean daily range of temperature, according to the observations of De Saussure, in the month of July, at the Col du Géant; and, of course, has reference to the *air* and *not* to the mass of the glacier.

0·00524,—he obtains, by the formula (2), numerical results, relative to the daily motion, sufficiently accordant (considering the nature of the problem) with those given by observation.

Without stopping to investigate whether a mass of matter having the structure and internal constitution of the glacial ice, would supply the same *mechanical conditions* as a metallic sheet or bar; it seems to me, that, whilst Mr. Moseley's reasoning may be perfectly applicable to ordinary cases of solid bodies resting upon inclined planes, its application as a cause of *Glacial motion* is peculiarly unfortunate. There are *physical considerations* which render this cause *totally inoperative* in the case of glaciers.

1. His theory is founded on a mistake as to a *physical fact*, arising from a strange oversight of the peculiar laws of *latent heat*. It is not a *fact*, that "variations in the quantity and temperature of the water," which percolates from the surface of the glaciers throughout their entire masses, would subject them to corresponding *variations of internal temperature*. For the water cannot permeate the capillary fissures without *melting* the ice, until its temperature is reduced to 32° Fahr. In other words, it is impossible for water to remain in contact with ice, *and both of them retain their respective physical states*, unless they are both at the temperature of melting ice. If warm water begins to descend, it *melts* the superficial ice and its temperature is speedily reduced to 32° Fahr. If the interior mass of ice is *below* the freezing point the water will *solidify*, and thus arrest the circulation:—so that, under any point of view, it appears to be physically impossible for the percolating water to convey the atmospheric variations of temperature into the interior of the glacial masses.

2. It is an immediate consequence of the laws of latent heat, referred to in the preceding paragraph, that ice *does not* change its temperature excepting for variations of temperature *below* 32° Fahr. Hence, during the *warm months*, the temperature of the glaciers (even at their surfaces) remains *invariably* at 32° Fahr., through all the fluctuations of atmospheric temperature: a conclusion abundantly corroborated by observation. And yet, experience shows, that their motion is *most rapid* during the *thawing season*:—that is, when their temperature must be *absolutely invariable!*

3. It appears very clearly from the thermometrical experiments of Prof. Agassiz, and from the observations of Prof. J. D. Forbes, that from 28° to 32° Fahr., is the habitual temperature of the great mass of a glacier; that the most rigorous nights in winter propagate an intense cold to but a very small depth; and that the temperature of the interior and lower parts is not sensibly lowered, even by the prolonged cold of winter. Hence it follows, that even during the *winter*, the cause which the theory demands (variations of temperature), is practically inoperative, when consid-

ered in relation to the great *mass* of the glacier.* Moreover, it is only during the *cold season*, that the superficial stratum of ice participates in the atmospheric oscillations of temperature; and yet, this is the season in which the motion of the glacier is *least rapid!* By some strange fatality, when the mechanical *cause* which the theory demands is *least active*, the *effect*, for which it is to afford an explanation, is at its *maximum* and *vice versa.*

The reference to the experiment of Mr. Hopkins is equally unfortunate for Mr. Moseley's theory. The following is the experiment alluded to:—"He placed a mass of rough ice, confined by a square frame or bottomless box, upon a roughly-chiselled flagstone, which he then inclined at a small angle, and found that a slow but uniform motion was produced, even when it was placed at an inconsiderable slope." "This motion," continues Mr. Moseley, "which Mr. Hopkins attributed to the dissolution of the ice in contact with the stone, would, I apprehend have taken place if the mass had been lead instead of ice; and it would have been but about half as fast, because the linear expansion of lead is only about half that of ice." (p. 67).

It is true, that a mass of lead might gradually have moved down the surface of the stone, from the expansions and contractions due to variations of temperature; but, would its motion have been *uniform?* or would it have been perceptible from hour to hour? As regards the motion of the mass of ice in this experiment, it is impossible to ascribe it to a similar cause; for Mr. Hopkins expressly states, that the motion took place *only* during the *melting* of the ice,† and consequently, when its temperature, by virtue of the laws of change of state, must have been *constantly* at the freezing point of water. There can be no satisfactory solution of the mechanical problem of the motion of glaciers, which does not contemplate the peculiar laws of *latent heat*, as well as the structure and other mechanical conditions of the glacial masses.

Athens, Georgia, Aug. 20th, 1855.

* But even if,—contrary to all observation and to all just theories of the propagation of heat,—we suppose the cold of winter to penetrate to any considerable depth into the glaciers, the vast amount of *latent heat* which must be extricated by the solidification of the first portions of water which permeate the cold mass after the thawing season commences, would very soon elevate its temperature to the freezing point; after which, its temperature could undergo no variation from the percolation of water.

† Vide Phil. Mag., 3d Series, vol. xxvi, p. 4, 1845

Art. XXX.—*On the Crystallization of Platinum from Fusion*; by J. W. Mallet, Ph.D.

Having recently prepared some bichlorid of platinum as a reagent by dissolving scraps of platinum foil, wire, etc., in nitro-muriatic acid, I poured off the yet strongly acid solution before the whole of the metal had disappeared, and washed and dried the remaining scraps. Among them there were five or six small beads of platinum which had been melted off from the end of a wire (of about $\frac{1}{40}$th inch in diameter) by the oxyhydrogen blowpipe flame. I was surprised to observe that these globules, which were spherical and quite smooth and brilliant before the acid had acted upon them, afterwards presented distinct traces of crystallization, resembling to the eye the little polyhedral beads of phosphate of lead which are obtained by fusing that salt before the blowpipe.

Some of the minute faces were plane or nearly so, but most of them were slightly rounded like those of many crystals of diamond. They presented for the most part the peculiar lustre of a metal with very minute striæ on the surface, but some of the facets were brilliant.

The prevailing form seemed to be the tetrakis-hexahedron, of which there was one very distinct example, and beside this, faces of the octahedron and *perhaps* of the cube with truncated angles—combinations of the cube with the octahedron—were recognisable on other beads.

Some of the globules were apparently *groups* of minute crystals, while one or two seemed to be distinct individuals.

The weight of the largest was not more than ·11 grm.

The crystalline faces were no doubt rendered visible by the *dissecting* action of the acid in the same way that the structure of a lump of alum may, as is well known, be brought out by partial solution in water—the chemically homogeneous mass offering at different points a greater or less resistance to the solvent, dependant upon the positions of these points with reference to the crystalline axes, just as the actual hardness of resistance offered to mechanical abrasion differs at different parts of the surface of a crystal.

The assumption of distinct crystalline structure by platinum under the circumstances mentioned, is remarkable from the extremely high melting point of the metal, the small quantity fused in each bead, and therefore the very short time in which each must have passed from the liquid to the solid state.

Art. XXXI.—*On an Index of Papers on subjects of Mathematical and Physical Science*; by Lieut. E. B. Hunt, Corps of Engineers, U. S. A.*

The history of science exhibits a continual tendency towards specialisation. The sphere of each laborer becomes more and more limited as the area of research is expanded and distributed into well defined specialities. The same thing is observable in scientific research which in the mechanical and chemical arts is familiarized under the name of subdivision of labor. The same advantages and disadvantages of the specialising tendency are equally observable in the domains of science and of manufactures. The restriction of investigation and of industry to limited fields of exercise, has the effect to produce the highest skill within those fields, at the price of narrowness of conception and privation of power concerning all other spheres of action and research. The man whose life is spent in heading pins becomes almost preternaturally skillful in the manipulations of that manufacture, but this man in any other sphere of industry is a blunderer and a bigot. So too the man of science who assiduously cultivates a chosen speciality becomes therein preëminent, but in so doing, he is in great danger of losing his grasp on those generalities which transcend his particular field, and of becoming not only impotent but bigoted relative to those branches of research which he has not pursued. As the microscopist restricts his field, but intensifies his vision within that field, so the cultivation of a speciality withdraws into a single study the diffused powers of the original mind. The microscopist and the specialist find the same difficulty in seeing the parts in their relations to the whole. No problem is more important to the scientific investigator than that of rightly coördinating his general and special culture. If generalisation too largely prevail, his life will probably be exuberant but fruitless. If a speciality absorbs all his powers, there will be an abundance of ignoble and innutritious fruit. If he knows how properly to combine the general and the special, he is likely to bear such rich and lasting fruit as has made the names of Newton and Leibnitz, Euler and La Grange, Cuvier and Agassiz, and all of their illustrious kindred, such unfailing sources of strength. The power to generalise and the power to specialise must coëxist in the true magnate of science. Great things may be done by men strong in either; but not the greatest. Whoever neglects general culture in his eager pursuit of the special, harms his own nature. It may be said that power in specialities can only thus be attained; that specialist devotees

* Read before the American Association for the Advancement of Science, at Providence, August, 1855.

are essential to the progress of science; that science like another Juggernaut, demands the sacrifice of the man himself to his chosen pursuit. Whoever thus reasons has already sacrificed his humanity to his speciality, and is but shouting the praises of Brahma. Nor is it easy to adopt a theory of scientific castes, which would make of the specialists a lower and necessary grade, whose business it is to gather materials for the generalising noble to arrange in their true order, around some latent principle which he alone is privileged to see. That some minds are thus noble by nature and by training is certain enough, but it is hard to believe that many who could be of any assistance as specialists, cannot also do some part however small, as the generalizers of their own field and of its relations to others.

The present age of science may with great propriety be called the monographic age. We have reached that period when every subdivision of research needs to be presented in the form of a monograph. While it is still possible for the student of any single subdivision to pursue it in all the original papers treating thereon, it is quite impossible for the investigator of a more extended area to study the contained and related subjects in their whole range of original papers. To him it is quite essential that the various subdivisions of research should each be digested by thorough masters of their component contributions, and brought out in their orderly, well balanced proportions. The true, the proven, the mature, has need to be separated by adept monographers from the false, the speculative, and the crude. The contributions of many successive investigators, each tending to place their common speciality on higher ground, and to bring it out in more perfect definition, have all to be digested by one who is qualified to take the judicial point of view, and to use aright the privilege of moderns, who, as Bacon says, are the ancients of knowledge.

The model monograph is one which presents the known substantive facts of the subject treated in their natural order and relations with all attainable clearness, completeness and brevity, and which gives such an insight into the nature of all original memoirs thereon, as will enable investigators to recur to such originals as may be important for their special purposes. Already the mathematical and physical sciences have hundreds of subjects needing to be monographed, while comparatively few are yet so adequately presented under this form, as not to need either a new monograph or a revision of the old. The great number of investigators now at work; the late remarkable increase of periodical publications and memoirs devoted to scientific subjects; the subdividing or fissiparous tendency of modern science and its signal proclivity to annex fields of empyrical practice and make them rational or strictly scientific; these and other causes have thrown

nearly all the provinces included within the present domains of science, into the precise condition to need the services of well furnished monographers.

An invaluable result of such a presentation of these subjects would be found in the aid which investigators would thus receive, towards keeping up their general scientific culture, without turning too much away from their specialities. There seems no possible way by which investigators can maintain the general and the special in correct and harmonious relations, except by means of an ever-increasing expansion of their monographic aids. As the realm of knowledge goes on yearly expanding and steadily growing more and more to exceed the possible grasp of any one mind, this need of a direct and disembarrassed presentation of all positive results stated as far as possible in the common language of science, will become continually more pressing. It will be only by the aid of monographs of various degrees of generality, that the entire domain of science can henceforth be at all surveyed by any single investigator. The narrowest fields, being first duly digested into monographs, will furnish the materials for monographs of higher generality, and from these, step by step, the ascent will be to that universal monograph, which shall draw within its compass all the high generalities of that great epic cosmos in which science will attain its far off final consummation. When physical science becomes thus organized and digested, we shall no longer be constrained to behold the ardent mind, borne away by generalising speculations beyond all basis of fact, or the far more revolting arid mind, cramped within a petty speciality, and disdainfully ignoring all the vast domain of vital generalities, simply because it transcends its own dwarfed and grovelling comprehension.

The only worthy type of scientific mind is that which at the same time pursues special and general truth, which can see grand generalities underlying the minutest phenomena, and which can trace in the loftiest generalities the simple convergence of an infinite series of kindred special phenomena. The entire character of the active investigating mind of the coming generation will be much influenced by this monographic tendency and need. The extent to which we shall digest into consistent wholes the now dispersed and fragmentary materials on a long array of subjects in mathematical and physical science, will almost measure the prospective prevalence of that just union of the generalizing and the specializing power whence alone can spring a true progress in scientific philosophy.

The views now presented, are essential preliminaries to a full understanding of the bearings and workings of an index of papers on mathematical and physical science. This subject is one which the naturalists have better appreciated than the mathematicians

and physicists. The proof of this will be seen in that laborious work which the genius of Agassiz inaugurated and which presents, in four octavo volumes, the titles of all known papers on Natural History and Geology up to its date. These titles are arranged alphabetically under the names of their authors, as is probably most convenient in the departments included, Natural History and Geology being essentially descriptive sciences, are less fitted for the classification of the subjects included, than the more abstract or logical mathematical and physical sciences. A logical arrangement by subjects, at least for Natural History, will scarcely now be proposed by those interested, though it is possible that Engelman's Bibliography of Natural History which distributes authors alphabetically under a limited number of general heads, may yet serve as a model for indexing papers or memoirs in the same field.

In proceeding to examine somewhat the subject of an index of all original papers in mathematics and physics, it may be well briefly to state the circumstances which have led me to consider it. As an Assistant in the Coast Survey, I had on several occasions to make special investigations, in which it was desirable to examine all good relevant authorities and original memoirs. How to do this was the question. To range over series after series, index after index from beginning to end, would surely bring to light all such papers. But this is a labor of truly appalling magnitude and not to be thought of for each minor research. It only remained to start with such papers as I chanced to know of, or could find by the few indexes of series at my command. Then following up all the references which could be gleaned from these sources, I could go on till they were exhausted. By that time any man of moderate patience would himself be exhausted, and indisposed to beat up, for farther game. Yet what guarantee is there in this process that the very best papers may not entirely escape one's knowledge. I found from experience just this result; nor do I suppose that any person can be sure of having examined all those printed papers on a given subject of physical science, which are essential to a full comprehension of its history and development. He may find a vast deal more than he likes to read and yet leave the best of all unknown. This is peculiarly true of those just entering on a career of research. Veterans bear in their memories some traces of the leading papers published during their lifetimes, on all subjects even likely to enlist their activity. But the neophyte has no such aid, and does not even know how to get at the most accessible memoirs. A great loss of time in turning over leaves for the want of specific references is common alike to young and old. Thus for instance when I wished to find all the published descriptions of automatic tide guages, I spent a great deal of time to find what could be read in five minutes, and am by no

means confident that I have seen the best description of the best foreign guage. Every persons experience must abound in such illustrations.

I did not many times repeat this experience of time-sacrifice before seeing the advantage of making this search for materials a general one and so once for all mastering the forces which were likely to be needed. I was thus led to form the plan of a systematic examination of all the principal series of scientific memoirs and periodicals for the purpose of extracting such titles of papers as I judged proper for a special index on subjects directly related to the various Coast Survey operations. This project met the hearty approval of the Superintendent, who has doubtless experienced its need more than any other person. He authorized my proceeding with its execution and has furnished every encouragement. I have already examined considerably over 1000 volumes of Memoirs, Translations, Scientific periodicals, &c., and it is my expectation to advance this index so far as to permit its being included in the Coast Survey Report of this year as an Appendix. It will be impossible for me to exhaust the field, but fortunately a supplement can at any time be added with the contributions of succeeding years, in which omissions from the first index may be supplied. This index will be arranged by means of a detailed classification under subjects, and will embrace various heads of interest to cultivators of physical science at large. Though the principle of relation is that of probable use in the Coast Survey operations, the ground covered will be of considerable extent.

Such a fragment alone, though rather laborious, I should not think worthy to bring before this Association, were it not that it had served to indicate a far more catholic plan and one which if executed will prove a signal benefaction to all cultivators of mathematical and physical science. This plan is simply an extension of the one already defined, so as to arrange in one general repository, all titles of papers on mathematical and physical science. The ground occupied by the index of Agassiz and his collaborators should be included so as to aggregate in the two works, references to all scientific researches. Engineering, Machines, Chemical, Mechanical and Artistic Technology would be invaluable additions, and should if possible all be embraced. All these titles being duly extracted, and when necessary annotated, would admit of classification into several volumes, each containing a connected group of subjects so that every investigator would find most of the references he would need in a single volume.

It was to somewhat such a shape that this plan reduced itself when I came to confer with Prof. Henry and Prof. Baird of the Smithsonian Institution. In the programme of objects proposed to be accomplished by that Institution is included a project of this

very character. Prof. Henry having as he declares found particular advantages from using the mathematical and physical index arranged by that clear-minded philosopher, Dr. Thomas Young, on a somewhat similar idea, was not likely to forget this among plans for the increase and diffusion of knowledge. It is indeed a plan for rendering accessible the knowledge already recorded, and as such eminently deserves the aid of that Institution. Were the work done and well done, the Institution would undoubtedly undertake its publication. But done it is not, and the question is how to accomplish its proper excution. Could I have commanded my own time, the performance of this work would have been a congenial labor. In spite of that precariousness of station which is my professional prerogative, I was much inclined to this undertaking and probably should have begun it, had not the assignment of triple public duties made it simple folly to venture it. Thus with the best intentions I am obliged to forego this expectation, and have brought the subject here in the hope that some one more favored by circumstances may be induced to undertake so needful an enterprise. I will gladly lend any aid in my power, to any one who is both fit and willing thus to work for the good of science. How far assistance and compensation might be allowed by the Smithsonian Institution in the execution of this plan is, of course, not for me to say. From the real value of the proposed work and the interest felt by the Smithsonian officers in its accomplishment, it is fair to infer that no reasonable aid would be denied which the means of the Institution would authorize. Its valuable collection of memoirs and transactions would be of peculiar value in this connection.

There is one excellent suggestion for which I am indebted to Prof. Baird. This is that an Index of American Scientific papers would be a useful and proper beginning for such an undertaking. The exceedingly scattered and anomalous vehicles through which American investigation have reached the present time, will make this portion of the search rather peculiar, and it is on this account much more needful. In truth, we do not know the real wealth of our own science, especially those of us who are young in such pursuits. Europe too is in a state of deplorable ignorance relative to our investigations, an ignorance which has considerable excuse too; for how can we expect foreigners to ferret out science from Patent Office and Coast Survey Reports or other public documents, Regents Reports, State Legislative documents, or indeed from any except the standard journals and memoir series. Thus there are very good reasons for an American Index, as preliminary to a general one, with which it could be regularly incorporated.

The work now proposed is certainly one of great labor. It will require several years and an examination of various libraries for its completion. Our own libraries will not offer all the

materials needed for its completion. English, French, German, Dutch, Spanish, Italian, Hungarian, Russian, Swedish and Latin series will need to be ransacked before the work is finished. A delicate exercise of judgment in accepting and rejecting titles will be required. There will be sundry questions such as these—Shall any anonymous papers be included? Shall papers in the library magazines be selected? Shall titles be given in full and literally, or cut down, modernized, translated or anotated? Shall translations be included? Shall reprints into more accessible series be referred to? &c. It has been my practice to include the date in each title, and to give the limiting pages of each memoir as an indication of the fullness with which its subject is treated.

The labor of classification will be one demanding a truly cosmopolite mind. Rightly to distribute the hundreds of subjects and always to maintain the truly logical catena of succession, will require a broad power of appreciation, such as few possess. Many plans of classification might be devised each having some advantages. But whatever system is adopted, an alphabetical index of subjects would make it easy of practical use. This study of classification would of itself be a most valuable research: for I am confident that any abstract or à priori plan would find the materials somewhat refractory and thus would undergo modification and amplification. The general alphabetical arrangement of titles by authors' names, would I think be quite unfit for this case. The greatest benefit of the whole plan would be in its bringing together references to all which has been written on each subject, and thus at once giving a clue to all the materials for perfect monographs. It is of far less importance in mathematics and physics to know the *who* than the *what*. But both can be sufficiently known by alphabetical arrangement of authors under subjects, and by an index of authors' names with references to all the subjects under which each has titles. Thus all the papers of any author and all the papers on any subject may be directly found.

I need not further enlarge on this plan. Should there be any who has capacity and courage to undertake the great enterprise indicated, farther hints would be superfluous for him. Should there be several who are willing to associate their labors, each taking certain series, and so together exploring the whole, it will not be difficult to concert such a general plan. The greatest difficulty of such an association would be in securing thorough coöperation and uniform execution. The means of classification would of course be by movable slips, and thus most incongruities of plan would be avoided by making the classification the work of one.

When we look at an individual labor so valuable as Poole's Index of Periodical Literature, we cannot doubt that the labor now proposed is destined ere long to enlist far greater industry and talent, and that if seriously undertaken it must succeed.

Dr. Young's Index, contained in the volumes of his works, would afford valuable aid towards conceiving the plan, though it is very far from perfect, and of too old a date to permit the continuance of its classification without much change. The proposed Index seems to be one of those undertakings which the current of events will render too indispensable not to be ere long begun. If so extended as to embrace Engineering, Machines, and the Technology of Art and Manufactures, (Chemistry in all its applications would of course be included,) it will become sufficiently valuable to many merely practical interests to enlist their active support. Our Patent Office might well afford to defray all the cost of such a work in those departments over which examinations for patents are required to be made.

If for a moment we conceive the result attained and the entire compass of reference to Mathematical and Physical papers brought into a systematic body, under specific subject-headings, we shall better realize its value. The course of investigation on any particular subject would be made simple and direct. By yearly supplements we might be kept informed of new papers beyond our ordinary range. The investigator would proceed to exhaust all the papers of value on any snbject in hand, and would know when he was done; that he would start thoroughly furnished for making additions to existing knowledge instead of wasting his strength on work already done. The preparation of a monograph would no longer involve a chartless roaming over a boundless sea, but our materials could be used in certainty of their completeness and in such succession as our convenience might dictate. Monographs thus made easy could not fail to cover field after field with unprecedented completeness and facility. We should in each branch be soon furnished with that clear synoptic presentation of all its important elements and results which would enable us to give each speciality its true value and relations on the general chart of scientific coördination. Our general views would keep pace with our special investigations, and our minds would attain that harmony of culture, characteristic of the well developed man. Alike versed in those grand generalities which form the groundwork of creation, and practiced in the study of our chosen fields of research, we should steer clear of fruitless speculations and of the bigotry of petty knowledge. Such would be the tendencies of the work proposed; what now remains is simply to do it, and for this nothing is wanting but the man or men. The benefit is for science among all nations; the benefactory reward will be a truly honorable distinction and a consciousness of usefulness such as few living men can claim. When we remember how the mighty dead and the honored living have given record to their best thoughts, in hope that the world would not willingly let them die, it becomes in us a deed of pious duty to retrace and render legible the inscriptions on these too neglected record tablets.

Art. XXXII.—*On the Geographical Distribution of Crustacea;* by James D. Dana.

(Continued from page 178.)

In continuing the extracts from the writer's Report on Crustacea, the notes relating to the genera and species characterising the several provinces exhibiting their relations and distinctive characters may be here for the most part omitted. The following are a few facts of special interest.

The relations of the Mediterranean region to Japan are mentioned by De Haan. The genera strikingly Mediterranean which occur in Japan, are *Latreillia*, *Nika*, *Caridina*, *Ephyra*, *Sicyonia*, *Achæus*, *Pandalus*, *Lysmata;* and the species of the last three, together with *Squilla mantis*, are probably identical, viz., *Pandalus pristis*, *Lysmata seticaudata*, and *Achæus Cranchii*, which last is at least hardly distinguishable, according to De Haan, from the *A. japonicus*. *Portunus corrugatus* is also closely like a Japan species, according to De Haan. The *Cycloes* of the Canaries is another of the Atlantic species, allying the Atlantic region to Japan, as above mentioned. Doclea is also an Oriental genus, represented in the Occidental kingdom by Libinia. It has but one described species out of the Oriental kingdom.

DECAPODA COMMON TO THE CELTIC PROVINCE AND THE MEDITERRANEAN.*

1. *Brachyura.*

Maia squinado, A.
Pisa tetraodon, A.
" lanata (Gibsii), A.
Achæus Cranchii, A.
Stenorhynchus phalangium, A.
Eurynome aspera.
Perimela denticulata, A.
Xantho floridus, A.
" rivulosus, A.
Pilumnus hirtellus.
Portunus pusillus.
" Rondeletii, A.
" depurator (plicatus), A.
" marmoreus.
" corrugatus, A.
" holsatus.
Carcinus mænas, A.
Portumnus latipes, A.
Gonoplax angulata, A.
Goniograpsus varius, A.
Pinnothera pisum.
Thia polita.
Corystes dentatus.

2. *Anomoura.*

Dromia vulgaris, A.
Porcellana platycheles, A.
" longicornis, A.
Bernhardus Prideauxii, A.
" Forbesii.
" streblonyx.
Clibanarius oculatus.
Galathea strigosa, A.
" squamifera.

3. *Macroura.*

Callianassa subterranea.
Arctus ursus, A.
Palinurus vulgaris, A.
Homarus vulgaris, A.
Nephrops norvegicus.
Crangon fasciatus, A.
" vulgaris.
" cataphractus, A.
Nika edulis, A.
Alpheus ruber, A.
Athanas nitescens, A.
Hippolyte varians, A.
" viridis, A.
Palæmon serratus, A.
Pasiphæa sivado.
Penæus sulcatus (caramote), A.

* Those species that are reported by Lucas from Algiers, are followed by the letter A. They also occur elsewhere in the Mediterranean.

The genus Xantho, in *X. rivulosus* and *X. floridus* here reaches its extreme cold limit in the Celtic Province. *Nephrops norvegicus*, although more properly pertaining to the next province north, occurs also within the limits of this; and it has even been taken in the Mediterranean. *Stenorhynchus phalangium* and *Portunus pusillus*, reach south into the Mediterranean and north to the Frigid zone; *Portunus holsatus*, *Galathea strigosa*, and *Porcellana platycheles*, south to the Canaries and north into the subfrigid. * * *

Turning Cape Agulhas, we soon come into a zoological world widely different from that of the Atlantic coasts. The coast immediately east to longitude 30°, belongs still to the temperate zone, and constitutes a distinct province, which we call the Algoa Province (from Algoa Bay), the length of which, measured from Cape Agulhas, is full five hundred and fifty miles.

Passing beyond this, we reach the Natal province, and here we recognise at once the seas of India and the Pacific Ocean. Krauss mentions eighty-one Natal species of Podophthalmia, not thirty of which are peculiar to this region. *Twenty* are found in the Indian Ocean, *eighteen* in the Red Sea, *thirteen* in Japan, *eight* in Australia, *five* in the Isle of France, besides *three* European species, and *three* American. We observe further that, twenty-two of the species of Podophthalmia occur in the Pacific Islands, among which are four species supposed by Krauss to be peculiar to Natal, viz., *Pagurus* (*Clibanarius*, D.) *virescens*, Kr., *Pagurus* (*Calcinus*, D.) *elegans*, *Galene natalensis*, Kr., *Platyonychus* (*Kraussia*, D.) *rugulosus*, Kr., all of which occur at the Hawaiian Islands.*

Of the *European* species, one is the cosmopolite *Gonodactylus chiragrus*, Latr. The others are *Alpheus Edwardsii*, and *Gammarus pulex*, Fabr. *Megalopa mutica* and *Hippolyte ensiferus*, also reported from South Africa, do not occur at Port Natal. The *American* are the cosmopolites *Goniograpsus pictus*, and *Gonodactylus chiragrus*, together with *Eriphia gonagra*, Edw. The *Sesarma reticulata*, Say, and *Plagusia tomentosa*, Lk., also South African, are not from Port Natal.

The wide distribution of the same species over the Oriental seas, from the African coast through the Pacific Ocean, is well shown in the following tables—a distribution due to the wide range of the tropical and subtropical waters. The *African* section here includes the east coast of Africa, with the adjoining islands; the *Indian*, the East Indies, southeast and east coast of Asia, and Eastern Australia; the *Pacific*, the Pacific Ocean.†

* The *Galene hawaiensis*, D., is so closely like the *G. natalensis*, that we believe there is not sufficient reason for considering them distinct.

† I. Fr. stands for Isle of France; E. I. for East Indies; Haw. for Hawaiian Islands; Jap. for Japan; Nat. for Natal; Feej. for Feejees.

SPECIES COMMON TO THE THREE SECTIONS, THE AFRICAN, THE INDIAN, AND THE PACIFIC.

1. *Brachyura.*

Parthenope horrida.—I. Fr., Red Sea; E. I.; Haw.
Atergatis limbatus.—Red Sea; E. I.; Feej.
Atergatis floridus.—Natal; E. I.; Tonga, Paumotus; Tahiti.
Carpilius maculatus.—I. Fr.; E. I.; Jap Samoa, &c., to Paumotus.
Carpilius convexus.—R. Sea: E. I.; Jap.; Feej., Haw.
Actæa hirsutissima.—R. Sea; Samoa.
Chlorodius niger.—R. Sea (N.); E. I.; Feej., Tonga, Samoa.
Trapezia ferruginea.—R. Sea; E. I.; Pacific.
Cymo Adreossyi.—R. Sea; E. I.?; Samoa, Tahiti.
Scylla serrata.—Natal; R. Sea; E. I.; Jap.; Samoa.
Lupa sanguinolenta.—Nat.; I. Fr.; R. Sea; E. I.; Haw.
Thalamita admete.—Nat.; R. Sea; E. I.; Samoa, Wake's, Haw.
Thalamita crenata.—Nat.; R. Sea (S.); E. I., Jap., Feej.
Cleistostoma Boscii.—Nat.; R. Sea; [E. I.?]; Feej.
Podophthalmus vigil.—I. Fr.; E. I.; Jap.; Haw.
Ocypoda brevicornis.—I. Fr.; E. I.; Tonga.
Acanthopus planissimus.—Nat.; E. I.?; Samoa, Tahiti, Paumotu, Haw. [also Madeira.]
Calappa tuberculata.—Nat.; I. Fr.; R Sea; E. I.; Feej., Tonga, Haw.
Calappa fornicata.—I. Fr.; E. I.; Feej.

2. *Anomoura.*

Pagurus difformis.—I. Fr.; E. I.; Feej.
Pagurus punctulatus.—E. I.; Haw.
Calcinus tibicen.—Nat.; E. I.; Samoa: Wake's, Tahiti, Paumotus, Haw.
Calcinus elegans.—Nat.; E. I.?; Wake's, Paumotus, Haw.
Aniculus typicus.—I. Fr.; Jap.; Wake's, Paumotus.
Clibanarius virescens.—Nat.; E. I.; Feej.
Cenobita rugosa.—Nat.; E. I., Jap.; Feej.; Samoa, Tonga, Paumotus.
Birgus latro.—I. Fr.; E. I., Jap.; Samoa, Swain's, Paumotus.

3. *Maeroura.*

Parribacus antarcticus.—I. Fr.; E. I.; Samoa, Paumotus.
Panulirus penecillatus.—R. Sea; E. I.; Pacific.
Hippolyte marmoratus.—E. I.; Pacific; Haw.
Stenopus hispidus.—I. Fr.; E. I.; Paumotus.

4. *Anomobranchiata.*

Pseudosquilla stylifera.—I. Fr.; Feej.; Haw.
Gonodactylus chiragrus.—Nat.; I. Fr., R. Sea; E. I.; Feej., Tonga.

Of the above species, a few occur in both the torrid and subtorrid regions of these three sections of the Oriental kingdom, that is, in the Erythrean, Natalensian, Indian, Liukiuan, Polynesian, and Hawaiian Provinces. These are:—*Lupa sanguinolenta*, *Podopthalmus vigil*, *Calappa tuberculata*, *Acanthopus planissimus*, *Calcinus tibicen*, *C. elegans*, and *Gonodactylus chiragrus*. *Grapsus pictus* is not included; it has not been reported from the eastern coast of Africa. The above list must be much increased as the species of the different regions are better understood. Some of the species have a range of over twelve thousand miles. Many species common to Natal and Japan or the Hawaiian Islands, are given in the above list. We add below a list of—

SPECIES COMMON TO THE NATAL AND THE LIUKIUAN (SOUTH JAPAN) OR HAWAIIAN PROVINCES OF THE SUBTORRID REGIONS, AND NOT YET OBSERVED IN THE TORRID REGION INTERMEDIATE.

Micippa thalia.—Nat. and Jap.
Xantho affinis, De H.—Nat. and Jap.
Xantho obtusus, De H.—Nat. and Jap.
Carpilius petræus, De H.—Nat, (I. Fr.) and Jap.
Charybdis granulatus.—Nat. and Jap.
Thalamita prymna.—Nat. and Jap.
Gelasimus arcuatus.—Nat. and Jap.
Gelasimus lacteus, De H.—Nat. and Jap.
Ocypoda cordimana.—Nat. and Jap.

Sesarma picta.—Nat. and Jap.
Sesarma affinis.—Nat. and Jap.
Kraussia rugulosa.—Nat. and Haw.
Galene natalensis.—Nat. and Haw.
Dromia hirsutissima.—S. Afr. and Haw.
Calappa spinosissima.—I. Fr. and Haw.
Doto sulcatus, Nat., Jap., and R. Sea.

The species of New Zealand, while somewhat related to those of southeastern Australia, have rather close relations to those of Chili, and also some resemblance to those of Britain. The genera *Ozius*, *Hemigrapsus* and *Chasmagnathus* are common to both New Zealand and the part of Australia referred to. The following genera characterize both Chili and North New Zealand:—*Cancer*, *Ozius*, *Cyclograpsus*, *Pagurist*es, and *Betæus;* and the *Cancer Edwardsii* and *Plagusia tomentosa* appear to be common to the two provinces, while the genus Cancer is not elsewhere known out of America and Northern Europe. *Palæmon affinis* of the Bay of Islands, as Edwards observes, is hardly distinguishable from *P. squilla* of the coasts of France and Britain. The species of *Portunus* in these southern seas are representatives of the most characteristic of European genera, and they belong rather to the cold temperate than subtemperate regions of the Australian and New Zealand seas. *Portunus integrifrons* is reported from Tasmania (Van Diemens Land). Ozius represents Xantho of the British Channel.

The occurrence in New Zealand of this representative of a cold water *Xantho*, of the *Palæmon affinis* so near a European species, of *Cancer* and *Portunus*, which are found together only in British seas, shows a striking zoological relation between these antipodes—a relation plainly dependent on the similar insular character and oceanic temperature of the two regions, Britain and New Zealand.

In the north Polar seas, the species have often a wide range, and probably pass from one ocean to the other through the Polar oceans. Thus *Crangon boreas*, *Carcinas mænas*, *Pagurus streblonyx*, *Hippolyte aculeatus*, are not only found on opposite sides of the Atlantic, but also in the North Pacific.

Origin of the Geographical Distribution of Crustacea.

The origin of the existing distribution of species in this department of zoology deserves attentive consideration. Two great causes are admitted by all, and the important question is, how far the influence of each has extended. The first, is *original local creations;* the second, *migration.*

Under the first head, we may refer much that we have already said on the influence of temperature, and the restriction of species to particular temperature regions. It is not doubted that the species have been created in regions for which they are especially fitted; that their fitness for these regions involves an adaptation of structure thereto, and upon this adaptation, their

characteristics as species depend. These characteristics are of no climatal origin. They are the impress of the Creator's hand, when the species had their first existence in those regions calculated to respond to their necessities.

The following questions come up under this general head:—

1. Have there been local centres of creation, from which groups of species have gone forth by migration?

2. Have genera only and not species, or have species, been repeated by creation in distinct and distant regions?

3. How closely may we recognise in climatal and other physical conditions, the predisposing cause of the existence of specific genera or species?

With regard to the *second* head, migration, we should remember, that Crustacea are almost wholly maritime or marine; that marine waters are continuous the globe around; and that no sea-shore species in zoology are better fitted than crabs for migration. They may cling to any floating log and range the seas wherever the currents drift the rude craft, while the fish of the sea-shores will only wander over their accustomed haunts. Hence it is, that among the Pacific Islands the fishes of each group of islands are mostly peculiar to the group, while the Crustacea are much more generally diffused.

A direction and also a limit to this migration exist, (1) in the currents of the ocean, and (2) in the temperature of its different regions. Through the Torrid zone, the currents flow mainly *from the east* towards the west; yet they are reversed in some parts during a certain portion of the year. But this reversed current in the Pacific never reaches the American continent, and hence it could never promote migration to its shores. Again, beyond 30° or 35° of north or south latitude, the general course of the waters is *from the west*, and the currents are nearly uniform and constant. Here is a means of eastward migration in the middle and higher temperate regions. But the temperature regions in these latitudes are more numerous than in the tropics, and species might readily be wafted to uncongenial climates, which would be their destruction; in fact they could hardly escape this. Moreover, such seas are more boisterous than those nearer the equator. Again, these waters are almost entirely bare for very long distances, and not dotted closely with islands like the equatorial Pacific.

In the northern hemisphere, on the eastern coasts especially, there are warm currents from the south and cold currents from the north. The former overlie the latter to a great extent in the summer and may aid southern species in northward migrations. Cape Hatteras is nearly the termination of the summer line of 70° (see Maury's Chart), a temperature which belongs to the subtorrid region in winter. On the China coast, at Macao, there

is a temperature of 83° in July, and in the Yellow Sea, of 78° to 80°. But such northward migrations as are thus favored, are only for the season; the cold currents of the winter months destroy all such adventurers, except the individuals of some hardier species that belong to the seas or have a wide range in distribution. Sea-shore Crustacea are not in themselves migratory, and are thus unlike many species of fish. Even the swimming Portunidæ are not known voluntarily to change their latitudes with the season.

The following is a brief recapitulation of the more prominent facts bearing on these points.

1. The distribution of individuals of many species through twelve thousand miles in the Torrid zone of the Oriental seas.

2. The very sparing distribution of Oriental species in Occidental seas.

3. The almost total absence of Oriental species from the west coast of America.

4. The world-wide distribution within certain latitudes of the species we have called cosmopolites.

5. The occurrence of closely allied genera at the Hawaiian Islands and in the Japan seas.

6. The occurrence of the same subtorrid species at the Hawaiian Islands and at Port Natal, South Africa, and not in the Torrid zone intermediate, as Kraussia rugulosa and Galene natalensis.

7. The occurrence of identical species in the Japan seas and at Port Natal.

8. The occurrence of the same species (Plagusia tomentosa) in South Africa, New Zealand, and Valparaiso; and the occurrence of a second species (Cancer Edwardsii (?)) at New Zealand and Valparaiso.

9. The occurrence of closely allied species (as species of Amphoroidea and Ozius) in New South Wales and Chili.

10. The occurrence of the same species in the Japan seas and the Mediterranean, and of several identical genera.

11. The occurrence of a large number of identical species in the British seas and the Mediterranean; and also in these seas and about the Canary Islands.

12. The occurrence of closely allied, if not identical, species (as of Palæmon) in New Zealand and the British seas; and also of certain genera that are elsewhere peculiarly British, or common only to Britain and America.

13. An identity in certain species of Eastern and Western America.

The following are the conclusions to which we are led by the facts.—

I. The migration of species from island to island through the tropical Pacific and East Indies may be a possibility; and the same species may thus reach even to Port Natal in South Africa. The currents of the oceans favor it, the temperature of the waters is congenial through all this range, and the habits of many Crustacea, although they are not voluntarily migratory, seem to admit of it. The species which actually have so wide a range are not Maioids (which are to a considerable extent deep-water species), but those of the shores; and some, as *Thalamita admete*, are swimming species.

II. The fact, that very few of the Oriental species occur in the Occidental seas, may be explained on the same ground, by the barrier which the cold waters of Cape Horn and the South Atlantic present to the passage of tropical species around the Cape westward, or to their migration along the coasts.

Moreover, the diffusion of Pacific tropical species to the Western American coast is prevented, as already observed, by the westward direction of the tropical currents, and the cold waters that bathe the greater part of this coast.

III. When we compare the seas of Southern Japan and Port Natal and find species common to the two that are not now existing in the Indian Ocean or East Indies, we hesitate as to migration being a sufficient cause of the distribution. It may, however, be said that driftings of such species westward through the Indian Ocean may have occasionally taken place; but that only those individuals that were carried during the season quite through to the *subtorrid* region of the South Indian Ocean (Port Natal, etc.), survived and reproduced, the others, if continuing to live, soon running out under the excessive heat of the intermediate equatorial regions. That they would thus run out in many instances is beyond question; but whether this view will actually account for the resemblance in species pointed out is open to doubt.

IV. When further, we find an identity of species between the Hawaiian Islands and Port Natal—half the circumference of the globe, or twelve thousand miles, apart—and the species, as *Galene natalensis*, not a species found in any part of the torrid region, and represented by another species only in Japan, we may well question whether we can meet the difficulty by appealing to migration. It may however be said, that we are not as yet thoroughly acquainted with the species of the tropics, and that facts may hereafter be discovered that will favor this view. The identical species are of so peculiar a character that we deem this improbable.

V. The existence of the Plagusia tomentosa at the southern extremity of Africa, in New Zealand, and on the Chilian coasts, may perhaps be due to migration, and especially as it is a south-

ern species, and each of these localities is within the subtemperate region. We are not ready however to assert, that such journeys as this range of migration implies are possible. The oceanic currents of this region are in the right direction to carry the species eastward, except that there is no passage into this western current from Cape Horn, through the Lagullias current, which flows the other way. It appears to be rather a violent assumption that an individual or more of this species could reach the western current from the coast on which it might have lived; or could have survived the boisterons passage, and finally have had a safe landing on the foreign shore. The distance from New Zealand to South America is five thousand miles, and there is at present not an island between.

VI. Part of the difficulty in the way of a transfer of species between distant meridians might be overcome, if we could assume that the intermediate seas had been occupied by land or islands during any part of the recent epoch. In the case just alluded to, it is possible that such a chain of interrupted communication once had place; and this bare possibility weakens the force of the argument used above against migration. Yet as it is wholly an assumption, we cannot rely upon it for evidence that migration has actually taken place.

VII. The existence of the same species on the east and west coasts of America, affords another problem, which migration cannot meet, without sinking the isthmus of Darien or Central America, to afford a passage across. We know of no evidence whatever that this portion of the continent has been beneath the ocean during the recent epoch. An argument against such a supposition might be drawn from the very small number of species that are identical on the two sides, and the character of these species. *Libinia spinosa* occurs at Brazil and Chili, and has not been found in the West Indies. *Leptopodia sagittaria*, another Maioid, occurs at Valparaiso, the West Indies, and the Canaries.

VIII. The large number of similar species common to the Mediterranean and British seas may be due to migration, as there is a continuous line of coast and no intermediate temperature rendering such a transfer impossible; and the passage farther south to the Canaries of several of the species is not beyond what this cause might accomplish. Still, it cannot be asserted that in all instances the distribution here is owing to migration; nor will it be admitted unless other facts throw the weight of probability on that side.

IX. But when we find the same Temperate zone species occurring in distant provinces, these provinces having between them no water communication except through the Torrid or Frigid zone, and offering no ground for the supposition that such a communication has existed during the recent epoch, we are led

to deny the agency of voluntary or involuntary migration in producing this dissemination. An example of this, beyond all dispute, is that of the Mediterranean Sea and Japan. No water communication for the passage of species can be imagined. An opening into the Red Sea is the only possible point of intercommunication between the two kingdoms; but this opens into the Torrid zone, in no part of which are the species found. The two regions have their peculiarities and their striking resemblances; and we are forced to attribute them to original creation and not intercommunication.

X. The resemblances found are not merely in the existence of a few identical species. There are genera common to the two seas that occur nowhere else in the Oriental kingdom, as *Latreillia*, *Ephyra*, *Sicyonia*, &c.; and species where not identical have an exceedingly close resemblance.

Now this *resemblance* in genera and species (without exact identity in the latter) is not explained by supposing a possible intercommunication. But we may reasonably account for it on the ground of a similarity in the temperature and other physical conditions of the seas; and the well-known principle of "like causes, like effects" forces itself upon the mind as fully meeting the case. Mere intercommunication could not produce the resemblance; for just this similarity of physical condition would still be necessary. And where such a similarity exists, creative power may multiply analogous species; we should almost say, *must*, for, as species are made for the circumstances in which they are to live, identical circumstances will necessarily imply identity of genera in a given class, and even of specific structure or of subgenera.

If, then, the similarity in the characters of these regions is the occasion of the identity of genera, and of the very close likeness in certain species (so close that an identity is sometimes strongly suspected where not admitted), we must conclude that there is a possibility of actual identity of species, through original creation. This, in fact, becomes the only admissible view, and the actually identical species between Japan and the Mediterranean are examples.

XI. When we find a like resemblance of genera and species between Temperate zone provinces in opposite hemispheres that are almost exact antipodes, as in the case of Great Britain and New Zealand, we have no choice of hypotheses left. We must appeal directly to creative agency for the peopling of the New Zealand seas as well as the British, and see in both, like wisdom, and a like adaptedness of life to physical nature. The Palæmon affinis of the New Zealand seas is hardly distinguishable from the common P. squilla of Europe, and is one example of this resemblance. It may not be an identity; and on this ac-

count it is a still better proof of our principle, because there is no occasion to suspect migration or any other kind of transfer. It is a creation of species in these distant provinces, which are almost identical, owing to the physical resemblances of the seas; and it shows at least, that a very close approximation to identity may be consistent with Divine Wisdom.

The resemblance of the New Zealand and British seas has been remarked upon as extending also to the occurrence in both of the genera Portunus and Cancer. It is certainly a wonderful fact that New Zealand should have a closer resemblance in its Crustacea to Great Britain, its antipode, than to any other part of the world—a resemblance running parallel, as we cannot fail to observe, with its geographical form, its insular position, and its situation among the temperate regions of the ocean. Under such circumstances, there must be many other more intimate resemblances, among which we may yet distinguish the special cause which led to the planting of peculiar British genera in this antipodal land.

The close resemblance in species and genera from Britain and New Zealand, and from Japan and the Mediterranean, and the actual identity in some species among the latter, proves therefore that, as regards the species of two distant regions, identity as well as resemblance may be attributable to independent creations, these resemblances being in direct accordance with the physical resemblances of the regions. As this conclusion cannot be avoided, we are compelled in all cases to try the hypothesis of migration by considering something beside the mere possibility of its having taken place under certain assumed conditions. The possibility of independent creations is as important a consideration. After all the means of communication between distant provinces have been devised or suggested, the principle still comes up, that it is in accordance with Divine Wisdom, to create similar and identical species in different regions, where the physical circumstances are alike; and we must determine by special and thorough investigation, whether one or the other cause was the actual origin of the distribution in each particular case. Thus it must be with reference to the wide distribution of species in the Oriental tropics, as well as in the European temperate regions, and the Temperate zone of the South Pacific and Indian Oceans.

XII. With respect to the creation of identical species in distant regions, we would again point to its direct dependence on a near identity of physical condition. Although we cannot admit that circumstances or physical forces have ever created a species (as like can only beget like, and physical force must result simply in physical force), and while we see in all nature the free act of the Divine Being, we may still believe the connexion between the calling into existence of a species and the physical circum-

stances surrounding it to be as intimate nearly as cause and effect. The Creator has, in infinite skill, adapted each species to its place, and the whole into a system of admirable harmony and perfection. In His wisdom, any difference of physical condition and kind of food at hand, is sufficient to require some modification of the intimate structure of species, and this difference is expressed in the form of the body or members, so as to produce an exactness of adaptation, which we are far from fully perceiving or comprehending with our present knowledge of the relations of species to their habitats.

When therefore we find the same species in regions of unlike physical character, as, for example, in the seas of the Canaries and Great Britain—regions physically so unlike—we have strong reason for attributing the diffusion of the species to migration. The difference between the Mediterranean and Great Britain may require the same conclusion for the species common to these seas. They are so far different, that we doubt whether species *created* independently in the two could have been identical, or even have had that resemblance that exists between varieties; for this resemblance is usually of the most trivial kind, and affects only the least essential of the parts of a species.

The continental species of Crustacea from the interior of different continents, are not in any case known to be identical; and it is well understood that the zoological provinces and districts of the land are of far more limited extent than those of the ocean. The physical differences of the former are far more striking than those of the latter. As we have observed elsewhere, the varieties of climate are greater; the elevation above the sea may vary widely; and numberless are the diversities of soil and its conditions, and the circumstances above and within it. Hence, as the creation of each species has had reference most intimately to each and all of these conditions, as well as to other prospective ends, an identity between distant continental regions is seldom to be found, and the characteristic groups of genera are very widely diverse. Comparatively few genera of Insects have as wide a range as those of Crustacea; and species with rare exceptions, have very narrow limits. Where the range of a species in this class is great, we should in general look to migration as the cause rather than original creation; but the considerations bearing on both should be attentively studied before either is admitted as the true explanation.

Throughout the warmer tropical oceans, a resemblance in the physical conditions of distant provinces is far more common and more exact than in the Temperate zone. And hence it would seem that we could not safely appeal to actual differences as an argument against the creation of a species in more than one place in the tropics. The species spread over the Oriental Torrid zone

may hence be supposed to owe their distribution to independent creations of the same species in different places, as well as to migration. Yet we may in this underrate the exactness of physical identity required in regions for independent creations of the same species. We know that for some chemical compounds, the condition of physical forces for their formation is exceedingly delicate; and much more should we infer that when the creation of a living germ was concerned, a close exactness in the conditions would be required in order that the creation should be repeated in another place. Infinite power, it is true, may create in any place; but the creation will have reference to the forces of matter, the material employed in the creation. The few species common to the Oriental and Occidental torrid seas seem to be evidence on this point. The fact that the Oriental species have so rarely been repeated in the Occidental seas, when the conditions seem to be the same, favors the view that migration has been the main source of the diffusion in the Oriental tropics.

As we descend in the order of Invertebrates, the species are less detailed in structure, with fewer specific parts and greater simplicity of functions, and they therefore admit of a wider range of physical condition; the same argument against multiplication by independent creations in regions for the most part different, does not, therefore, so strongly hold. As we pass, on the contrary, to the highest groups in Zoology, the argument receives far greater weight; and at the same time there are capabilities of migration increasing generally in direct ratio as we ascend, which are calculated to promote the diffusion of species, and remove the necessity of independent creations.

Migration cannot therefore be set aside. It is an actual fact in nature, interfering much with the simplicity which zoological life in its diffusion would otherwise present to us. Where it ends, and where independent creations have taken place, is the great problem for our study. This question has its bearings on all departments of Zoology; but in few has migration had the same extended influence as in that of Crustacea. Molluscs, if we except oceanic species, are no travellers, and keep mostly to narrow limits.

XIII. There is evidence in the exceedingly small number of Torrid zone species identical in the Atlantic and Indian Oceans, that there has been no water communication across from one to the other in the Torrid zone, during the period since existing species of Crustacea were first on the globe.

XIV. As to zoological centres of diffusion for groups of species, we can point out none. Each species of Crustacea may have had its place of origin and single centre of diffusion in many and perhaps the majority of cases. But we have no reason to say that certain regions were without life, and were peo-

pled by migration from specific centres specially selected for this end. If such centres had an existence, there is at present no means by which they may be ascertained. The particular temperature region in which a species originated may be ascertained by observing which is most favorable to its development: we should thus conclude that the *Ranina dentata*, for example, was created in the subtorrid region and not the torrid, as it attains its largest size in the latter. By pursuing this course with reference to each species, we may find some that are especially fitted for almost every different locality. Hence, we might show, as far as reason and observation can do it, that all regions have had their own special creations.

The world throughout all its epochs in past history, has been furnished with life in accordance with the times and seasons, each species being adapted to its age, its place, and its fellow species of life.*

ART. XXXIII.—*Notice of the Geological position and character of the Copper Mine at Bristol, Connecticut;* by Professors B. SILLIMAN, Jr., and J. D. WHITNEY.†

Situation.—This well known mine is situated in the north part of the town of Bristol, Connecticut, about three miles from the Canal Railroad, and four from the centre of the town, where the Station House of the Hartford and Fishkill Railroad is located.

The persevering enterprise with which this mine has been explored, the rich character of its ores, the extent and costliness of its former workings, and the general impression, lately much strengthened, that it still possesses great wealth in its ore channels, all continue to make it a subject of peculiar interest to all who feel the national value of such investments and experience. Nor is its economical value diminished by the scientific interest attaching to the peculiarly complex nature of the deposit.

Geological position and nature of the deposits.—This mine is worked in a "contact deposit," between the sandstone of the Connecticut River Valley, now supposed by geologists to be of Liasic age, and the older metamorphic rocks, commonly called primary, but in all probability of the palæozoic system.

The direction of the line of contact of the two formations at the mine is about NE and SW.

* The abstracts in this Journal from the author's Report, relating to the Geographical Distribution of Crustacea, which are here concluded, are contained in volumes xvi, xviii, xix and xx. The map illustrating the subject of Oceanic temperature, the Zones and Zoological Provinces, is in volume xvi.

† This article is taken from a "Report of an examination of the Bristol Copper Mine in Bristol, Conn., by Professors B. Silliman, Jr., and J. D. Whitney, Aug., 1855. New Haven: pp. 32. Printed for private distribution."

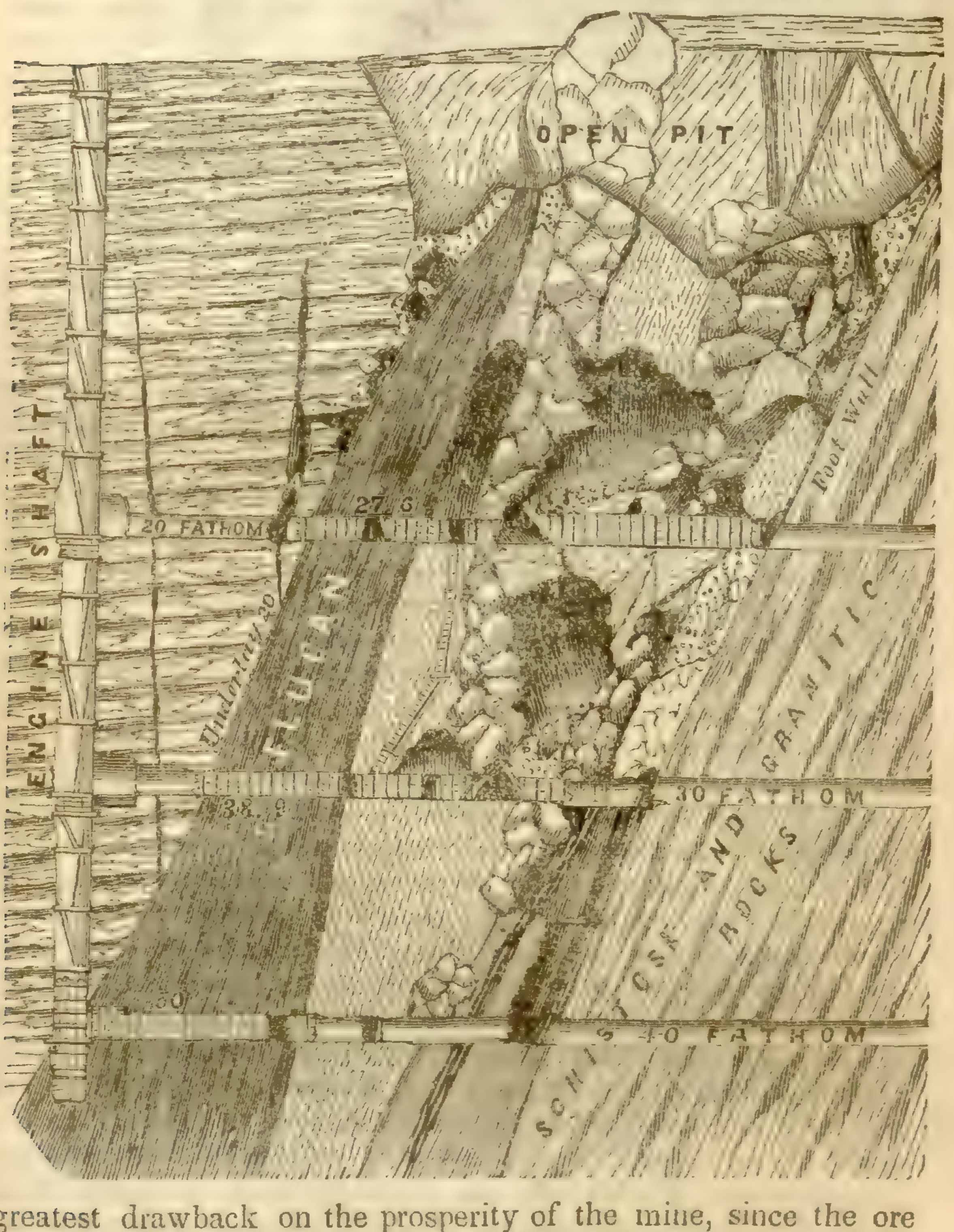
The linear extent of ground known to be metalliferous, by excavations made at various times, is between 1100 and 1200 feet. The principal workings are, however, concentrated in a space of about 600 feet north and south. There is good reason to believe that the metalliferous ground does extend at least 1500 feet, and perhaps considerably further. The mine is opened to the depth of 40 fathoms (240 feet) by an engine shaft, 6′ × 8′ in dimensions, sunk in the sandstone, and passing out of this rock into the great ore-channel at this depth. This is at present the only working shaft of the mine, and is used for hoisting the ore, for the pit-work, and as a ladder-road to the levels. The width of the ore-ground from east to west, or at right angles to the line of contact, at the engine shaft, as exposed in the cross-cuts and levels which have been driven, is about 120 feet, and this width is maintained at the lowest depth yet reached in the mine, and the same is true for a longitudinal extent of about 500 feet, although it appears from the examination of the workings and what is known of their history, that the greatest concentration of metalliferous matter was on the surface in the vicinity of the engine shaft.

The character of the ore-ground or metalliferous belt is exceedingly complex.

Up to a recent period, however, the workings were chiefly in a series of micaceous and hornblende slates, sometimes passing into gneiss, and including large irregular "horses" of granite, which rock appears to have formed segregated masses, lying rudely parallel with the bedding of the schistose rocks. The strike and dip of these, however, is found throughout the mine to be very irregular, and there is evidence in the confused character of the ground, as well as in the slip joints and polished surfaces of the rocks, that motion of the various beds upon one another has taken place along lines of upheaval of limited extent and varying direction. The distribution of the ores in the metalliferous ground now under consideration, is found to be as irregular as is the structure of the ground itself. They consist principally of the vitreous, with some variegated ore, and a comparatively small amount of copper pyrites. The magnificent crystallizations found at the Bristol mine, especially of the vitreous ore, have given the locality a world-wide celebrity among mineralogists. In general these ores are found occurring in bunches and strings, which although preserving, usually, an approximate parallelism to the line of contact of the formations, cannot be traced continuously for any considerable distance. Hence the irregularity of the workings, especially in the upper levels, which have been extended in upon various branches of ore, or in search of others supposed to exist in certain directions, without any particular system or previously conceived plan. This has been the

TRANSVERSE SECTION OF BRISTOL COPPER MINE.

Showing the Contact of the Sandstone with the Crystalline Rocks, and the Position of the Great Flucan and Ore-ground of the Mine. Scale 60′ to 1″.



greatest drawback on the prosperity of the mine, since the ore ground was too wide to be all taken down by the miners, and the distribution of the branches of ore in it was too irregular to admit of their being found without occasional expensive excavations in dead ground. In general, throughout the mine, a tendency to a concentration of the ore around the masses of granite may be remarked, and the latter are not unfrequently well filled with strings and bunches of ore, especially near their exterior.

The limits of the ore-ground to the west, or in the direction of the older rocks, the sandstone being to the east of the contact-

line, have never been well ascertained, and must be somewhat irregular, as would be expected from the nature of the deposit. Eustis's level, going north from the 20-fathom cross-cut, has been driven along a well-marked and regular wall, which has been commonly called the foot wall of the mine. This dips, at an angle of 62° to the East, preserving a very near parallelism with the line of contact of the formations. The same kind of wall is seen in the 30-fathom level immediately beneath, where it is observed in the 20; but in neither level can it be traced for more than one or two hundred feet. There is nothing in the nature of the wall to indicate that ore might not be found beyond it, as appearances similar to this are observed in the ore-bearing ground of the mine. The excavations indicate that there was a tendency of the metalliferous matter towards this limit, and that there is not much probability of finding large bunches of ore beyond it. It is worthy of remark, however, regarding the ore-ground within the limits just described, that the average distribution of the vitreous copper is very uniform, so much so that for a long period it has been found profitable to crush and work over nearly every stone from this belt raised in working the mine.

The ore-ground just described was from the time the mine was first opened up to a recent period, considered as the only source in which the yield of copper was to be sought. Not long since, however, attention began to be called to a belt of soft rock, which lies next adjacent to the sandstone, and, of course, between it and the metalliferous belt just described. This belt is known as the "great flucan," (see diagram on previous page,) and it consists of a talco-micaceous slate completely disintegrated and softened by some chemical agency, with the exception of a few bands and nodules of hard rock, but which are of very limited extent, compared with the softer portion. The flucan contains throughout its whole mass vitreous and other ores of copper, usually disseminated through it in small particles, so as hardly to be visible to the naked eye, but occasionally concentrated into bunches and strings of considerable size. The width of the great flucan is 27 feet in the 20-fathom level, but it increases rapidly as the workings descend, being respectively 38 and over 50 feet, in the 30 and 40-fathom level. If this increase in width were to continue in depth, the flucan will meet the so-called foot-wall of the mine at or near the 80-fathom level, occupying in this place a width of about 120 feet. But that such an increase in its dimensions continues downwards indefinitely, seems hardly probable; it is more likely that it will be found to be a lenticular mass swelling out to great dimensions, and then gradually contracting again. The mass of flucan has been traced on the surface and opened at various points. At the distance of 700 feet to the southwest, from the engine-shaft, it has near the surface an inconsiderable width,

although it may increase in descending: to the north, however, it holds its width as far as it has been explored. Jilbard's level was driven in this direction from the 20-fathom cross-cut, for a distance of over 300 feet, and the flucan there found to have a width of 41 feet. Still further to the north it has been cut at the surface, beyond Ives's level, but its width in this place was found to be at 30 feet from the surface much diminished.

The deposits of ore are not entirely confined to the primary. Within the sandstone, which at the line of junction is so broken up and irregular, that its stratification can with difficulty be made out, a small seam of ore was cut in the 20-fathom cross-cut from the engine-shaft. It has been driven on both-ways from the cross-cut to a distance of about 160 feet to the north, and 120 to the south. The upper part of this course of ore is still standing, showing a narrow seam of quartz rich in ore. The level to the north is at present inaccessible: to the south the ore appears to run out at a distance of about 50 feet from the cross-cut. (This seam of ore is shown in the cross section at A.)

History of the mine.—This locality has been known for a long period as a place furnishing copper, and was opened to a very limited extent many years since. In 1836 Mr. G. W. Bartholomew made an open cut in a spot where indications of ore were abundant, and the following account of its appearance is given in 1837, by Prof. C. U. Shepard, State Geologist, who saw it at that time:

"The trench has a nearly north and south course, and extends for twenty feet somewhat obliquely across alternations of vertical layers of granite and soft mica slate. The layers of granite are from one to two feet in thickness, while those of the slate are generally much less. It was found on getting below the surface that the green malachite and brown iron ochre diminished in quantity, and that the rocks were less decomposed. Yellow copper pyrites began to make its appearance in the slate and variegated copper in the granite. The integrity of the rocks and the abundance of the ore increased regularly as the workmen descended, till at the bottom the granite presented several almost continuous veins or layers of ore which in places had a thickness of two inches."—*Report*, p. 47.

Mr. Percival says: "The Bristol Mine is chiefly opened in a large vein of a coarse reddish sub-talcose granite, adjoining which the gneissoid micaceous rock, just referred to, is also sub-talcose or sub-chloritic, with decomposed greenish chloritic seams and nodules, and with more or less copper disseminated."—*Report*, p. 77.

The property subsequently passed into the hands of Mr. A. R. Miller, who drove various shallow levels near the surface, exploring for copper. While in his possession it was visited and re-

ported on by Prof. Silliman, Senior, of Yale College, in 1839, and upon this report funds, to a small amount, were obtained in England for prosecuting the work.* But the adventurers being unable to sustain the enterprise, the explorations were subsequently continued by Mr. L. C. Ives, who erected a small steam engine, and sunk the present engine shaft in the sandstone to the 20-fathom level. During this time, and prior to 1847, considerable shipments of the ore had been made to England, but exactly what quantity we have no means of ascertaining. It appears, however, from the information obtained from the last named proprietor, that it could not have been less than 100 to 125 tons.

It was not, however, until 1846–7, that the mine was opened and worked to any considerable extent. At that time it passed into the hands of Mr. Hezekiah Bradford and his associates. Since that period a large amount of money has been expended here, and over eighteen hundred tons of copper ore have been raised and sold. In 1851 the management of the mine passed into the hands of Mr. Henry R. Sheldon, as agent for the present owners. Mr. Sheldon (who is still the manager) has had the satisfaction of seeing the mine rise from a state of great depression in its affairs and prospects to a condition of prosperity and encouraging profits, with the certainty that by a judicious and moderate outlay, it can be made to return the whole cost of its plant and extraordinary expenses, with a wide margin of profit to the adventurer.

The value of the "great flucan" in ore appears from the average of all the trials which have been made upon it for a long period, including the actual results of stamping and crushing to be over 3 per cent of ore having 30 per cent copper. In a calculation made upon the probable future product of the mine, it is assumed that the average yield will be 2½ per cent. The material composing the flucan is so soft and easily removed that no gunpowder is required in mining it. As soon as it is exposed to the action of air and moisture it *runs* as the miners express it and becomes like a micaceous clay. It weighs a little more than 150 pounds to the cubic foot. In a careful examination of the fresh material, it is not difficult to see that it is chiefly the feldspathic portion of the original material which has undergone decomposition. The outline of the feldspathic portions is plainly discovered by the friable mass of mingled silica and clay of a white color loosely filling the space occupied by the feldspar. Small garnets, of a deep red color, and small black tourmalines are constantly to be seen when the material is washed—being apparently almost the only substances beside the vitreous ore of copper which have escaped the powerful decomposing influence which has produced this great zone of altered material.

* This Report was in confirmation of one made in October, 1838, by Prof. C. U. Shepard, and which we have never seen.

In reflecting upon the probable cause of the decomposition of this extensive bed of talco-micaceous rock the suggestion naturally arises that it has probably a close connection with the injection of the trappean rocks among and beneath the sandstones of the secondary. This event it is certain was a subaqueous one, and the vast efflux of steam of high tension produced by the fiery injection of the molten trap, among rocks saturated with water must have left its traces in chemical changes produced by so powerful an agency. The irruption of the copper ores may have been and probably was an earlier event of segregation. But if the cause adduced for the decomposition of the flucan is the true one, it is pretty certain that the same decomposed material will be found at the greatest depth to which the mine will ever be explored. As it is now proposed to sink a shaft in the sandstone to the depth of 600 feet and to connect it at every 60 feet with the flucan by a cross-cut, we shall have the opportunity of studying these phenomena in detail as the explorations proceed. It is rather remarkable that the line of actual contact between the sandstone and the crystalline rocks is seldom visible and has still more rarely been carefully explored. We do not know of another example where this important feature in our geology has been explored with so much care as in the case before us.

Many statistical data embodied in our report and which are chiefly of economical value, are omitted here. It is proper to remark, however, that an important item in the present and future success of this mine is the employment of new and improved machinery, the most important of which are the ore separators and patent jigs, invented by Mr. H. Bradford. The separator is a shaking table of entirely new form and construction, on which by the rapid reciprocal motion of a copper plate the ore is by aid of water and gravity, thrown over one edge while the waste is projected over the other. After giving in detail the statistics of their operation, our report holds the following language:

Undoubtedly Bradford's separators are the most important mechanical invention ever produced for dressing what are called slime ores. The principle upon which they are constructed is truly philosophical, and has been most ingeniously applied by the talented inventor to the production of this machine. Like all accurate instruments they require careful adjustment in all particulars, but once adjusted they are completely self-acting, so that one attendant can easily oversee at least twenty of them.

Our "Report" concludes with the following observations—

In reviewing the condition of the Bristol Mine at the present time, the following are the most noteworthy points:

1. The mine is one of the few in the United States which is now worked at a profit.

2. There is a large extent of ore-ground known to exist in the mine, and which can, although of a low percentage of ores, be worked at a profit.

3. There is good stoping ground in the solid parts of the mine, which can be profitably wrought.

4. The widening of the flucan in descending, and the appearance of the solid ore-ground at the lowest points yet reached by the workings, render it a safe and advisable matter to open the mine at a considerably greater depth. Since there it is certain that the flucan may be wrought with profit, and there is good reason to believe that good leaders of ore will be cut in the solid part of the ground, and to hope that the various branches may unite into one master lode or form rich bunches of ore at the crossings of the east and west branches.

5. The position of the dressing works is such in relation to unsettled ground, that their position must be changed, or preparations made for doing so immediately.

6. A new engine shaft must be commenced to cut the lode at 80 or 100 fathoms, and must be pushed forward as rapidly as possible.

7. The ore stuff now on the surface will go far towards returning the cost of all necessary improvements requisite to place the mine in a state of great productiveness, and with proper care the old works can be kept up with undiminished returns during the erection of the new machinery.

August 26, 1855.

Art. XXXIV.—*On our Sense of the Vertical and Horizontal, and on our Perception of Distance;** by Lieut. E. B. Hunt, Corps of Engineers, U. S. A.

Phenomena are often unobserved or difficult of analysis, by reason of their connection with our habitual acts of consciousness. Conclusions derived from a life-long experience of our particular conditions of existence, are practically ranked as intuitions. From infancy onwards, each waking moments' perceptions go to impress these conditions upon us, as almost of absolute necessity. Our relations to space and to the earth, in their external or apparent form, are so incessantly subjects of our consciousness, that we come to regard them without special consideration or question. As a mind which has spent its habitual energies in pursuing analytical geometry, conceives coördinate axes, almost as matters of course in every investigation, so do we all, habitually experiencing the action of gravitation, come to con-

* Read before the American Association for the Advancement of Science, at Providence, August, 1855.

sider its influence, our up and down, our horizon and our general geometrical circumscription, essentially as matters of course, as intuitional perceptions, and as subjects with which reason has little concern. It is doubtless true however that we acquire such fundamental ideas from continuous experience, just as positively, as we do any other elements of our practical knowledge.

Our ideas of the vertical and horizontal and our sense of distance, have thus become so firmly established by our life-long experience, that their origin, antecedent to our philosophising exercise of reason has become much obscured. Yet they are clearly capable of rational analysis, and their component elements can be made subjects of experiment and variation. As introductory to some observations of this nature which I have made, I will present such general views as these and other experiences have suggested.

The vertical and horizontal are our habitual coördinate axes of external geometrical perception. Always ourselves, at the origin of coördinates, we refer objects seen to their respective positions, by an instantaneous perceptive location relative to these axes. We combine a mental estimate of distance with an angular reference to the vertical and horizontal axes and plane. In the horizontal plane, our perceptive usage seems to be to refer to a central line in our field of view, by angular coördinates. When we attempt to embrace the whole horizon in one mental sweep, we for the most part refer objects to the North and other compass lines; this however is a more deliberate process, and our consciousness of its steps is comparatively complete.

In the attempt to define the sources of our perception of the horizontal and vertical, two stages of causation have occurred to me as chiefly concerned in its formation.

The first of these depends on our sensibility in each part of the body and limbs, to the action of gravitation; an obscure physiological sensation of weight and pressure, in a particular direction, throughout the sensitive components of our bodily frame, a sensation which is probably greatly dependant on the circulating fluids. A specific difference of sensation notifies us of the precise inclination of each portion of our system, and could we be blind-folded and sustained in any position in the air, without any distinct consciousness of a point of suspension, we should probably still have a clear perception of the vertical at any instant. But in our ordinary position of observation, the body notifies us of the slightest variations of direction in the gravity action on each of its parts, and in the act of running down hill over rough grounds or on points of rocks, it is particularly apparent how extremely delicate is this appreciation. In this exercise our ability to combine the appreciations of ground inequalities, of our forward and downward motion of the force of gravity, and of that diminution of effective gravity due to the increasing rapidity of bodily

descent, quite suffices to make us wonder. This diminution as I have repeatedly observed, when commencing a rapid run down hill subtracts much from the force or shock of the footfalls. The skill of jugglers in balancing poles, and of rope dancers in self-balancing, are illustrations of the same gravity sensibility. When we consider the unconscious skill of the fingers of the pianist or compositor, and that in all habitual muscular acts, the tendency is to a mechanical or *quasi* involuntary action, it will not seem singular that our continuous life-long perceptions of gravitation in all our sensitive parts, should assume an obscure or latent form in our minds which would cause all its results or effects to seem intuitional. I ascribe that sense of the vertical within the limits of the body whence we pass to an acquaintance with the same in the external world, entirely to this trained gravity sensibility. In the remarkable case of John Metcalf, who though blind, was long and successfully employed in locating new roads over Derbyshire peak, this gravity sensibility must have become particularly acute, as to this only could his skill be fundamentally ascribed.

The second causal stage in our perception of the horizontal and vertical in the external world will be found to be chiefly visual. The testimony of the body to the direction of gravitation makes us aware of the angle between the vertical and the optic axes, by reason of the permanent organic relation between the body and these axes. In our ordinary or erect position, the mean of this angle or its value, when a perfect muscular equilibrium prevails, is a right angle, and any departure from this mean is indicated by the sensations of the various muscles, which cause the elevation or depression of the optic axes. Thus our sensations alone suffice to indicate to us the vertical, and a visual horizon perpendicular to it; and these sensations, checked and verified by a life-long experience, acquire an accuracy of indication which would hardly be anticipated. To these sensational coördinates, we spontaneously refer all external objects. There is very much also in our ordinary perspectives, to suggest and indicate the true horizon. Water, as the broad sheet, the winding river and the tumbling brook, level bottom lands and broad prairies, continuous hill crests, isolated knolls, either directly present horizontal planes or give such logical indications of them as the experienced eye can readily interpret, so as to derive with much accuracy a true general horizon. Indeed, we seem never to look over an extended view, without almost instantly defining with more or less precision our supposed horizon. Our previous experience from travelling over ground, doubtless enters directly in our definite location of this mental horizon. Are we in a valley, we pass our horizon under most of the objects seen. Are we in a middle point of view, we pass it in a mean position. Are we on a hill top, we pass it tangent to the general sweep of the landscape.

Thus experience is made to check and qualify the sensational indications, and we fix our horizon by a compound act of sensation and judgment from past experience.

In our mental estimate of distance, which we habitually make for all objects seen in perspective, there is doubtless a kindred combination of sensational and reflective elements. Sensation gives us a direct measure of distance for objects near at hand, while for remote objects, the element of judgment enters largely. Our normal vision being binocular, the distance between the optical centres of the two eyes, becomes a base line of constant length, to which all external distances are referred, with a precision, constantly decreasing, both outwards and inwards, from the interior limit of distinct vision. The convergence of the optic axes on each external object, is a direct result of voluntary muscular action and its particular value is indicated by a specific sensation in the eyeball muscles. Thus the third dimension of perspective is directly represented by a sensation connected with the perceiving eyes, and adjacent distances are referred, by a real triangulation, to the interocular base line. Being thus sensationally advised of the distances of objects, as far as this base continues to be an appreciable quantity relative to the perspective distances, we have, as it were, a considerable circle around us, within which we perceive, with an accuracy diminishing with distance, the absolute nearness or remoteness of objects. From the distances thus known, we pass on by an act of comparison or judgment, to those of more remote objects, where the direct sense of distance fails us. Here too, as for near objects, we doubtless receive some aid from the conscious focalization of the eye and from the gradation in the clearness of definition of outlines. All our experience in appreciating distances, gained by travelling over ground, and checking our estimates by actual comparison, also involuntarily comes to our aid. Our knowledge of the ordinary and absolute sizes of the various classes of objects seen, will besides greatly affect our rational estimates of distance. Thus sensation gives a foothold on perspective distances; comparison, or a species of continuous secondary triangulation, extends our visual scope; and experience brings in the judgment to correct false estimates.

If these views be correct, monocular vision should be much less accurate than binocular in the estimation of distances near at hand. The lack of the interocular base would make the process of perspective gaugeing almost exclusively an act of judgment. The only sensational element in the former, would be that of a conscious focalization of the eye, which could hardly be of much importance. Thus, I should conclude, that persons with only a single effective eye, would be bad judges of distance, especially among near objects. How the fact stands, I have had no means of ascertaining.

I will now instance a few observations or experiences, tending to confirm the general views here presented.

1. In passing around a particular curve of the railroad from Worcester to Boston, I have twice observed that the whole landscape on the concave side seemed to dip strongly towards the southeast, while from the adjoining views and the position relative to the ocean, I am satisfied that the real dip could not be nearly so great as the apparent. This I conceive to result from a composition in the body, of the centrifugal action with gravity, which inclined the sensible vertical from the curve centre, and so apparently lifted the sensible horizon, or depressed the actual ground relative to the same. This instance seems to show clearly the dependence of our horizon on our gravity sensibility.

2. From the summit of Crow Nest, the mountain just north of West Point on the west bank of the Hudson, the view southward presents two distinct reaches of the river in the same glimpse, which seem entirely separate bodies of water, by reason of a mountain spur, which severs all apparent connection at a distance of some six miles. From the height of over 1200 feet, you look down on the river not over a mile distant and can readily fancy this portion an elongated lake, embosomed in granite hills. Some fifteen miles below, a second apparent lake is seen, and this, despite your knowledge that it is a lower part of the same river, seems elevated above the water beneath you by perhaps 250 feet. This delusion is due, I conceive, to a dip in the apparent southern horizon, caused by our not appreciating the elevation of our point of view, relative to the hill ranges over which we look and so passing our horizon too low among the south Highland peaks. The boldness of the south face of Crow Nest hides that side from view when on the top, and so makes us think the summit of view much lower than it really is. Indeed from a point some 200 feet lower on the river face, the height above the river seems greater than that from the summit, probably because you there see the whole connecting slope. Thus in passing our visual horizon as a kind of general tangent plane, we give it a south dip, which seems to lift the distant water. This illustrates the power of our general visual habit over our gravity sensibility and our distinct knowledge.

3. From Jones' Hill, which forms the west bank of that Sleepy Hollow, where the exemplary Ichabod Crane achieved immortal fame, the west view embraces a general slope of three-fourths of a mile to the Hudson, then the broad Tappan sea, and the west bank beyond, half Palisades and half Highlands. On a quiet afternoon of last year, I there saw a thick fog so drape the west bank as wholly to hide it from view, yet leaving the east bank and about half of the river in full clear sight. The fog looked extremely sky-like and shut down in a clear line on the water.

When I simply looked sensationally on this scene, the lower fog line on the water seemed like a distant water horizon, and all above seemed but a cloudy sky. So strong was this impression, as not to be essentially impaired by seeing, half buried in the fog, the sails of a river sloop, which looked as if resting in the cloud sky. This illusion seemed to lift the hill on which I stood, to a height greatly exceeding its actual elevation. The strong likeness of the fog boundary to a sea horizon rotated the apparent horizon, so as to make it dip much below the real one, towards the west; thus overcoming both my gravity sensibility and a familiar knowledge of the locality.

4. A few weeks since, in an early morning, when standing on the bow of a sail-boat in Newport harbor, I observed, during a thick fog, a singular apparent distortion of the water surface. The boat seemed to rest in a bowl or hollow of water, some four rods in radius. This bowl seemed bounded by a gently curved swell, which ran tangent to the apparent remote horizon. Thus the water around me seemed some four or five feet lower than the horizon water. This appearance I suppose to have resulted from the fog line along the water, looking like a dim distant horizon, and thus bringing the apparent limiting horizon circle much too near. This would make the apparent horizon dip below the true, and would thus give a false apparent level to the water as far in as to that point, where in looking down on it, the true level could be distinctly seen. The true and delusive levels seemed joined by a curved swell, the curve evidently depending on the height of the eye and the density of the fog. This observation, like the preceding, illustrates the power of our idea of a water horizon.

5. If we look at the moon when a little above the horizon, its apparent enlargement may, by the simple act of shading with the hand all objects below the moon, be proved to be due in great part to the effect of intervening objects in magnifying our idea of its distance. The disc seems then to undergo a striking contraction of size. On dropping the hand, the moon seems again rapidly to enlarge. I think the effect of this masking is to give an apparent diameter not quite so small as when the moon is on the meridian, but it does not lack very much of this result. It will be noticed too, that when we see the sun or moon in the horizon, through forest branches, especially when we are travelling in a railroad car, they seem remarkably dilated. The great number of intervening branches and trunks is clearly the direct cause of this illusion.

This reasoning applies directly to explaining the apparently ellipsoidal form of the sky dome. Seeing along the horizon many objects on which the binocular or perspective sensibility fastens to elaborate the sense of distance, the horizontal axis is

much elongated in appearance, while the vertical axis, having no intervening objects to aid, is seemingly shortened. Doubtless the cause assigned by Euler, or the absorption of light by its oblique passage through the lower or misty strata in the vicinity of the horizon, may have a considerable agency in this phenomenon.

6. I was once of a party to observe Saturn through the West Point equatorial, and the several persons observing declared that the apparent size of the disc was, according to one, the size of an orange, according to another, as large as a cart-wheel, &c. In this case, the lack of perspective makes the visual angle the sole guide, except the obscure sense of focalization by which the instrumental pencils may affect the eye. Hence the utter vagueness of all estimates of apparent size, which must depend on a baseless imagination as to distance.

7. In sailing on Boston harbor, I have twice seen the phenomena of diverging and converging rays, or what is commonly called "the sun drawing water," both towards the sun in the west and towards the point symmetrically below the horizon in the east. Clear as it was rationally, that these two ray systems were but the opposite perspective views of the same parallel beams of light, I could not by any effort make them seem so sensationally. The opposite ray systems would not blend and I could not make them seem connected. The visual thinning of each beam near the perpendicular made the connection wholly invisible and so completed the illusory projection of the beams on the sky-dome. Hence an apparent widening of each beam, as it receded from the sun or from the opposite point. This whole appearance shows a complete subjection of the mental to the apparent or pictorial.

8. The great power of the two eyes to determine distances by the convergence of their axes, was strikingly shown when I was observing the reflection of a gas-burner globe from a street window of the room in which I was during the decline of day in New York. When looking with both eyes, this reflection seemed firmly established just by a branch of a tree, some ten feet outside the window. On closing one eye, I found it easy to transfer the image entirely across the street, by a mere exercise of imagination. In fact, when using only one eye, the rigid stability of the binocular vision was gone entirely, and the apparent distance seemed almost to become the subject of direct volition.

9. If in looking out over a landscape, we, by lying down or otherwise, bring the line of the two eyes into a vertical position, the scene will be found to undergo some remarkable changes. Hills will seem to recede into the distance, so as to look like far off mountains. The perspective will be found to lose very much of its relief and we seem rather to be looking at a picture on a

huge panoramic canvas, than on an actual receding perspective. Our sense of the horizon is quite impaired and a strange whimsicality of look becomes conspicuous, if we observe thus steadily for some time. In looking thus over a sheet of water, bordered by land varying from 3 to 20 miles distant, I noticed the same hollowing of the water near by, as before mentioned in the case of fog on the water. This was clearly due to the same apparent recession of near objects towards the horizon bound, and a consequent dip of the apparent horizon. It is clear too that by bringing both eyes into the same vertical, the interocular base becomes nullified for distance gaugeing in the horizontal direction. Hence objects seem to recede to one general cylinder around you, and the perspective perception is exceedingly impaired. The case is thus almost one of monocular vision. The study of these changes of scenery by our simple rotation of the interocular base, is very interesting and instructive. It shows, not only the influence of this base in forming our perspective distribution of distances, but that our sense of the horizon is very dependent for its firmness and precision on our gravity sensibility in our ordinary erect position.

Various other facts illustrative of the general views presented might be instanced, but the above must now suffice.

Art. XXXV.—*Biographical Notices of Edward Forbes and others;* by William John Hamilton, Esq.*

Gentlemen.—It now becomes my duty, in accordance with the practice uniformly adopted by my predecessors in this chair, to address to you some observations on the losses we have sustained during the past year, and it is with unfeigned sorrow that I have first to allude to one whose name can never be mentioned in these rooms without emotion. I need not say that I allude to Edward Forbes, who was endeared to us by every tie of social friendship and scientific merit, and who has been snatched away from us at the moment when he had reached the highest position his ambition could have coveted, or his admiring countrymen could have bestowed on him. Scarcely had a few short months intervened since he had been called by the universal voice of the science of Great Britain to fill the chair of Professor of Natural History in the University of Edinburgh, during which we were still regretting his departure from the metropolis, before we were astounded and overwhelmed by the unexpected announcement of his death. We felt not only individually that we had lost a valued friend, but that

* From the Anniversary Address of the President of the Geological Society of London, of Feb., 1855.

those anticipations of a brilliant scientific career, justified by the position he had attained and by the opportunities placed within his reach were doomed to bitter disappointment. These reflections are most painful, and, were I to follow my own inclinations, I would willingly forego all further allusion to the subject; but such a course would be a betrayal of duty towards our departed friend, and would disappoint the justly-founded expectations which you entertain of hearing a more detailed account of the distinguished and amiable man whose loss we so deeply deplore.

Edward Forbes was born in the Isle of Man, in the month of February, 1815. He evinced, at a very early age, an unusual taste for the study of natural history, and began to form a small museum when scarcely seven years old. A few years later he commenced his geological studies with the perusal of Buckland's 'Reliquiæ Diluvianæ,' Parkinson's 'Organic Remains,' and Conybeare's 'Geology of England,' exhibiting at the same time a more than usual taste for drawing.

He visited London at the age of sixteen, and was then engaged in studying drawing under Sass, but this was not enough to occupy his eager and ardent mind. He proceeded in 1831 to Edinburgh, where he devoted his whole time and energies to the pursuit of his favorite subject of natural history, while professing to overcome his repugnance for the study of medicine, the ostensible object of his matriculation. But medicine as a profession had no charms for one whose whole soul was filled with a love of the beautiful, and with an intense admiration of the works of Nature in every varied form. He cultivated his taste for natural history under the able teaching of such men as Professors Jameson and Graham. He delighted particularly in the botanical excursions of the latter, who was accustomed periodically to lead forth his pupils to the Highlands; thus making Nature herself, in her truest and loveliest garb, afford the practical illustrations of the teaching of the class-room.

At this period of his life, scarcely a year passed without some botanizing or dredging excursion, and long before he arrived at manhood, he had made himself well acquainted with the Fauna of the Irish Sea, on the shores of his native island. At the age of eighteen, in company with a fellow student, he made an excursion to Norway, where he spent some weeks exploring the wild and romantic districts of the country, adding to his zoological and botanical observations. Already at this time Edward Forbes began to direct his attention to botanical geography, the forerunner of those deep and philosophical views respecting the geographical distribution of the Flora and Fauna of the world which he subsequently developed, and which constitute one of the most interesting and leading features of all his writings.

In 1835, Edward Forbes visited the Alps; in 1837 he was prosecuting his studies at Paris under Beudant, Prévost, Geoffroy St. Hilaire, and De Blainville, and in May of the same year we hear of him at Algiers; the result of this expedition was an account of the land and freshwater mollusca of Algiers and Bougia, published in the 'Annals of Natural History' for May, 1839.

With the same view of prosecuting his researches in natural history, he visited Styria and Carniola in 1838, his remarks on which were published in the 'Proceedings of the Botanical Society.' In the summers of 1839 and 1840 he delivered at Edinburgh, whilst still a student, a course of scientific lectures on zoology, as well as one of a more popular nature, in which he pointed out the bearings of zoology on geology. I mention this as a subject of peculiar interest to us, as indicating the commencement of those views which, by their subsequent development and their growing importance in the hands of Edward Forbes, have exercised such a beneficial and practical influence on the study of geology.

The time was now fast approaching when Edward Forbes was to find a wider sphere for the exercise of his brilliant genius. In 1841 he published his 'History of British Star Fishes and other Echinoderms,' a delightful volume, charmingly illustrated by his own pencil and from his own designs. There are many in this room who will recognize in these illustrations the same ingenious and playful fancy, and the same ready pencil which never allowed a sheet of paper to lie unused before him, while he had a chance of transferring to it the humorous and graceful forms which he realized without an effort, and almost without a thought. In this same year he obtained the appointment of naturalist to H. M. surveying ship *Beacon*, Captain Graves, then employed in completing the survey of the coast of Asia Minor and the adjacent islands: an appointment more suited to his tastes and to his talents could not have been devised. He had here full play for the prosecution of his favorite pursuits of botany, zoology, and geology. Already well acquainted with the flora and fauna of the European Continent and their geographical distribution, he had now an opportunity of tracing their further extension to the East, and of examining the first appearance of the Oriental *facies* which they put on in the eastern portions of the Mediterranean. Nor was Edward Forbes the man to neglect such an opportunity. During this and the following year he pursued his botanical and zoological researches with unwearied energy, assisted by Captain Graves, who omitted no opportunity of enabling his scientific friend and companion to avail himself of every occasion for observation which the service afforded. It was during his various excursions in the *Beacon* and her boats, that Edward Forbes followed out those researches with the dredge, amongst the islands of the

Ægean Sea and on the adjacent coast of Asia Minor, which alone would have immortalized his name. At the same time he neglected no occasion of studying the geology and botany of the regions which he visited; but the dredge and its results will ever remain the chief glory of this expedition. The results of these researches were made known to the public in the 'Report on the Mollusca and Radiata of the Ægean Sea, and on their distribution, considered as bearing on Geology,' made to the British Association at their meeting at Cork in 1843. From this report it appears that the data on which it was founded were entirely derived from personal researches during a voyage of eighteen months in the Ægean, when but a few days passed by without being devoted to natural history observations. The calculations were based on more than 100 fully-recorded dredging operations in various depths from 1 to 130 fathoms, and in many localities from the shores of the Morea to those of Asia Minor. And with that modesty which ever characterized Edward Forbes in all his works, he adds, that the merit of the results is mainly due to Captain Graves. The chief objects of the report, as stated by the author, were, "to give an account of the distribution of the several tribes of mollusca and radiata in the Eastern Mediterranean, exhibiting their range in depth and the circumstances under which they are found; to inquire into the laws which appear to regulate their distribution, and to show the bearings of the investigation on the science of geology."

I shall not attempt to give an analysis of this valuable report; I shall content myself with reminding you of some of the more important conclusions, as bearing on geological investigations, which are embodied in it. The most important fact which has resulted from them respecting the development and distribution of animal and vegetable life in the depths of the ocean is, that of the almost uniform occurrence of particular species in particular zones of depth below the surface. This distribution of marine animal life is determined by three primary, modified by several secondary, influences. The primary are climate, sea composition, and depth; of the many secondary influences, the most important is the character of the sea bottom. According as rock, mud, sand, weedy or gravelly ground, prevails, so will the number of the several genera and species vary. The outline and geological nature of the coast is also an important feature in modifying the marine fauna. Other secondary influences are tides and currents, the influx of fresh water, &c.

We have then a full description of eight well-marked regions of depth in the Eastern Mediterranean, each characterized by its peculiar fauna, and when there are plants by its flora. These regions are distinguished from each other by the association of the species they severally include. Certain species in each are found

in no other, several are found in one region which do not range into the next above whilst they extend to that below, or *vice versâ*; a certain species have their maximum of development in each zone, being most prolific in individuals in that zone in which is their maximum, and of which they may be regarded as especially characteristic. Every zone has also a more or less general mineral character, the sea bottom not being equally variable in each, and becoming more and more uniform as we descend. Again, the deeper zones are greater in extent, so that whilst the first or most superficial is but 12, the eighth, or lowest, is above 700 feet in perpendicular range; its horizontal extent increases in a somewhat similar proportion. Another significant feature is, that as we reach the eighth zone, the number of species and of individuals diminishes as we descend, pointing to a zero in the distribution of animal life as yet unvisited. Species disappear in depth which do not seem to be replaced by others. From other observations the following general inference is deduced; that the extent of the range of a species in depth is correspondent with its geographical distribution.

But these eight regions are themselves the scene of incessant change; not only are the depths modified by the addition of fresh matter, but the very animals themselves, by their own increase, so modify the nature of the sea bottom as to render it unfit for their own existence, until a new layer of sedimentary matter, uncharged with living organic contents, has formed a fresh soil for similar or other animals to thrive on. It is impossible to overlook the importance of these observations in explaining many of the daily recurring phenomena which are brought under the notice of the geologist; in the last observation we may see an explanation of the phenomenon of interstratification of fossiliferous and non-fossiliferous beds.

I must refer you to the report itself for an account of the phenomena which would be presented to us were the bottom of the Ægean Sea to be elevated and converted into dry land, or to be filled up by a long series of sedimentary depositions. He concludes by observing that, "supposing such an elevation to have taken place, a knowledge of the association of species in the regions of depth would enable us to form a pretty accurate notion of the depth of water in which each bed was deposited, A beautiful illustration of this argument is given from observations made on the island of Santorin, and under different circumstances the contrary observations might be made; the geologist is thus enabled, by a careful examination of the successive overlying groups of species, to ascertain whether in any given locality brought under his notice, the sea bottom was being elevated or depressed."

But I have already dwelt too long on this report; I must hasten to other scenes in the life of Edward Forbes. During his

stay in the Mediterranean, he made several excursions into Lycia, where he had an opportunity of combining his love of art with the pursuit of natural history. On one occasion, in company with Mr. Hoskin, they discovered and fixed the sites of two of the Cibyratic cities. A second excursion undertaken with the Rev. Mr. Daniell and Captain, then Lieutenant, Spratt, was still more important; the sites of eighteen ancient cities hitherto unknown to geographers were explored and determined, and the names of fifteen were identified by inscriptions found amongst the ruins. During this expedition Mr. Daniell fell a victim to the malignant malaria of the country, and the life of Edward Forbes himself was at one time in danger. Indeed there can be little doubt that at this time were sown the seeds of that disease which has eventually deprived us of his services. He, however, gradually recovered, and was on the point of proceeding to Egypt and the Red Sea on a dredging excursion, when he was informed that he had been elected to fill the Chair of Botany in King's College, vacant by the death of Professor Don. He returned immediately to England, and, on the 8th May, 1843, delivered his inaugural lecture in that institution.

But previously to this event, Professor Forbes had become intimately connected with this Society. At the close of 1842, Mr. Lousdale, who for so many years had been the curator of our museum, resigned his office in consequence of the state of his health. In the report of the Council read at the Annual General Meeting on February 17, 1843, I find the following passage, after alluding to the loss sustained by the resignation of Mr. Lousdale: —"In recording the election of his successor, the Council cannot omit to congratulate the Society on having secured the services of such a distinguished naturalist as Mr. E. Forbes." I may appeal to the recollection of every member of the Society for a confirmation of my statement, that the expectations then entertained, great as they unquestionably were, were more than fulfilled by the manner in which Edward Forbes conducted the business entrusted to him during the period that he held this important office. The report of the Museum Committee for 1844 will show how his labors were appreciated by the Council. But before the close of the same year his talents as a naturalist and a palæontologist called him to a more extended sphere of action. On the establishment of the Museum of Practical Geology in connection with the Ordance Geological Survey under the direction of Sir H. De la Beche, Professor Forbes was appointed palæontologist to that institution, and resigned the curatorship of the museum of this Society. On the removal of the Museum to Jermyn Street he was appointed its Professor of Natural History.

Here then his talents had full space for their development, and Edward Forbes was not slow in bringing to bear on his numerous

avocations the knowledge he had so industriously collected. Combining as he did a lively and vivid imagination with a mature and well-disciplined judgment, he was enabled to employ with effect that power of generalization and abstraction which he so eminently possessed. His enlightened and comprehensive views on the numerous branches of natural history which he cultivated, and which were founded mainly on his own experience, caused him from henceforth to be looked up to as one of the first of British naturalists, and the works which he now published bear ample testimony to his well-founded reputation. Nor was it in England alone that his merits were recognized. In France, in Germany, in Italy, wherever men of science were to be found, the name of Edward Forbes was equally acknowledged as deserving a place in the first rank of scientific merit.

Towards the end of 1846, he published with Lieut., now Captain Spratt, an account of his travels in Lycia, a work in which we are at a loss to know whether most to admire the admirable details of archæology and art, or the equally graphic description of the botany, geology, and zoology which it contains. About this time appeared in the Proceedings and Transactions of our Society, his monograph on the South Indian Fossils sent to this country by MM. Kaye and Cunliffe and the Rev. W. H. Egerton. The report itself independently of the description of the fossils, is short, but it is not the less important, and is eminently characteristic of the author. He points out the general resemblance of the *facies* of the fossils to that of the Cretaceous period of Europe, and more particularly the lower portions of that series. His arguments are drawn rather from similarity, than from identity of species; a subject to which he had particularly directed his attention during his researches in the Ægean Sea. The report is preëminently suggestive, and I would particularly mention that portion of it which refers to the occurrence in these Cretaceous beds of certain forms which are usually considered as characteristic of Tertiary formations, and which very forms are now found in their greatest assemblages living in those eastern seas,—a fact, which, he observes, goes far to support the theory, that genera, like species, have geographical birthplaces as well as geographical capitals.

About this time, also, he wrote one of the most remarkable contributions to the science of Geology, which has appeared in this country. It is published in the first volume of the Memoirs of the Geological Survey of Great Britain, and is entitled "On the Connexion between the Distribution of the existing Fauna and Flora of the British Isles, and the Geological changes which have affected their area." "In this work," to use words already printed, "the happy combination of great botanical and zoological knowledge is made to bear on some of the most intricate inquiries with regard to the age and relationship of the rocks of Great Britain."

Mr. Horner, when President of this Society, has borne his ready testimony to the merits of this work, when he says in his Anniversary Address in 1847, that this Essay "is an admirable example of the light to be derived from other branches of natural history in the prosecution of geological inquiries; of the application of animal and vegetable physiology, and a knowledge of the habits and distribution of animals and plants to the elucidation of very difficult problems in geology." Mr. Horner, in the Address from which I have quoted these words, has given an admirable account of this interesting and attractive memoir, so suggestive as it is of great and enlightened views. I will therefore here only observe, that the principal theory which it is the object of this Essay to establish, is based on the assumption of the existence of *specific centres*, that is, of certain geographical points from which the individuals of each species have been diffused, involving their consequent descent from a single progenitor, or from two, according as the sexes might be united or distinct. Prof. Forbes further declares, as his opinion, that the "abandonment of this doctrine would place in a very dubious position all evidence the palæontologist could offer to the geologist, towards the comparison and identification of strata, and the determination of the epoch of their formation." Having assumed the truth of the doctrine of specific centres, the problem which he proposes to solve is the origin of the assemblages of the animals and plants now inhabiting the British Islands. Within this limited area he considers that the united labors of British naturalists have shown that there are a great number of animals and plants which are not universally dispersed, but are congregated in such a way as to form distinct regions or provinces. The vegetation, for instance, presents five well-marked Floras, four of which are restricted to definite provinces, whilst the fifth, besides exclusively claiming a part of the area, overspreads and commingles with all the others.

Prof. Forbes considers that, of the three given modes by which an isolated area may become peopled by animals and plants, "immigration before isolation" of the area was the mode by which the British Isles have chiefly acquired their existing flora and fauna, terrestrial as well as marine, and that it took place subsequently to the Miocene epoch. It follows from this argument, that previous to the isolation of this area, it must have been in direct union with those portions of the European continent the floras of which are shown to be identical with one or other of the five floras of the British Isles. I will briefly mention the five distinct floras which he has noticed, and the districts with which he considers they prove our former connexion.

1. *The West Irish Flora.*—The high lands in the North of Spain present the nearest point where a vegetation occurs identical with that which is characteristic of the mountainous district

of the west and southwest of Ireland. Consequently, at some period or other, continuous dry land must have existed from the coast of Spain to that of Ireland.

2. *The Devon Flora*, connected with that of the Channel Islands and the neighboring parts of France.

3. *The Kentish Flora.*—The vegetation of the southeast of England is distinguished by the presence of a number of species common to this district and the opposite coast of France.

4. *The Alpine Flora.*—On the tops of some of our most lofty mountains, particularly in Scotland, are plants not found elsewhere in the British Islands, but which are identical with those of the Scandinavian Alps, thus pointing to a former connection in that direction.

5. *The General Flora.*—This universal flora is almost identical as to species with the flora of central and western Europe, and may be properly styled Germanic.

The arguments by which these views are maintained are clearly and satisfactorily developed, but must be read and studied to be appreciated. That portion of the paper, however, which relates to the distribution of the marine plants and animals now inhabiting the British seas is still more deserving of careful study. The account of the distribution of the British Mollusca is particularly so; it contains a mass of information on the subject, not to be found, at the time of its publication, in any one work, and of the greatest value to the student of Tertiary geology. I will only mention one or two of the more interesting points with which the memoir concludes.

"That the flora and fauna, terrestrial and marine, of the British Islands and seas have originated, so far as that area is concerned, since the Miocene epoch.

"The greater part of the terrestrial animals and flowering plants now inhabiting the British Islands are members of specific centres beyond their area, and have migrated to it over continuous land, before, during, or after the glacial epoch.

"All the changes before, during, or after the glacial epoch appear to have been gradual and not sudden, so that no marked line of demarcation can be drawn between the creatures inhabiting the same element and the same locality during two proximate periods."

Of the many scientific papers of great merit which Prof. Forbes subsequently published, in our own and other Journals, I will only allude to one, which in this room cannot be passed over in silence. In his paper 'On the Fluvio-marine Tertiaries of the Isle of Wight,' published in the 9th vol. of our Quarterly Journal, the result of the laborious investigations of several months, he has established, on data which cannot be questioned, the true order of superposition of the upper tertiary beds of that typical locality,

correcting the errors of previous inquirers, and confirming a suggestion made by Mr. Prestwich, that the strata composing a part of Hempstead Hill were probably higher than any beds hitherto noticed. The result of Prof. Forbes's inquiries has been to show that, taking the Whitecliff Bay section for an example, the Headon Hill beds, instead of constituting the highest portion of the series, are overlaid by several other distinct formations, consisting of the St. Helen's or Osborne beds and the Bembridge series, the latter consisting of several distinct divisions, all characterized by peculiar fossils, chiefly, however, freshwater or brackish. He has, moreover, distinctly ascertained that the Hempstead Hill series constitutes another subdivision overlying the uppermost bed of the Bembridge series, and characterized by a fresh set of fossils. "Thus," to use the author's words, "we find that the fluvio-marine Eocenes of the Isle of Wight are more than twice as thick as they have hitherto been regarded, and that the additional beds are even of greater geological importance than those hitherto recognized."

The remarkable feature in this section is, that from the Barton series upwards there is no break in the series of deposits; and as Prof. Forbes identifies the Hempstead series with the middle, and possibly the upper Limburg beds of Belgium, he is logically led to the conclusion, that the Limburg beds, and consequently the Weinheim beds of the Mayence basin, which are unquestionably of the same age as the Middle Limburg, must be also Eocene. Other continental beds are also referred to as necessarily belonging to this Eocene period. This is not the place to offer any criticism on Edward Forbes's conclusions, but I may perhaps hereafter allude to the question, for the purpose of testing whether there may not exist some flaw in the argument, by which so many of the younger continental beds are drawn into this Eocene vortex.

During this period Prof. Forbes was not only most industrious with his pen, but he was unwearied in his arrangement and classification of the vast accumulation of fossils collected by the Ordnance Geological Survey, and now exhibited in the Jermyn Street Museum. He was no less active in the field with his hammer and his note-book. He not only explored various parts of England, Wales, and Ireland, but he visited with the same observant eye and comprehensive glance, many portions of Belgium and of France, carefully comparing their various aspects and phenomena, and procuring materials for his philosophical generalizations.

It is unnecessary for me to remind you of the satisfaction with which we hailed his appointment to the Presidentship of this Society, looking forward to the influence of his profound knowledge of palæontology on the future progress of our science. But scarcely had he occupied his chair for half the allotted term, when

the death of his old master, Prof. Jameson, was announced in this metropolis. The universal voice of science was not slow in recognizing Edward Forbes as the man who, above all others, both as a naturalist and a geologist, was most fitted to succeed him. At the same time, I am bound to say, that while we were all ready to congratulate him on the prospect of thus reaching the highest goal which a true naturalist could desire, we looked forward with regret to the prospect of his removal from our circle. Nor was this grief altogether free from a feeling of shame, that this vast city, with its wealth, its display, its riches, its public and private associations, its great collections, its lavish expenditure, and in many respects its unbounded liberality, could propose no prize, no reward to the scientific man worthy to be placed in competition with that offered by the northern capital. Little did we then imagine that the regret we felt at his departure from amongst us was destined to be so soon merged in another, as overwhelming as it was altogether unexpected. Little did we imagine that the fond anticipations of a long and glorious career for our friend, in which we then indulged, were doomed to be so speedily destroyed.

Prof. Forbes was appointed to the vacant Chair of Natural History in the University of Edinburgh. He had thus obtained the great object of his life. An intimate friend, writing in one of the Edinburgh journals, says that he considered all his plans, excursions, observations, &c., as preparatory to this one object. During his active and laborious life, all his hopes and future plans pointed to Edinburgh as the only appropriate place for developing that vast amount of natural history acquirement he had obtained. There he looked forward, amongst other things which his eager fancy had prepared for him, to the formation of a magnificent museum, arranged according to that system which for years he had been zealously maturing. Nor can there be any doubt, but that with the liberal support of government, assisted by that of private individuals, he would have been enabled in a few years to carry out his plans. But alas! scarcely had he reached that goal which he had spent his whole life in endeavoring to attain, and which he was anxiously preparing to adorn with all the ornaments of science collected from every quarter of the globe, when he was suddenly carried off by the inscrutable decrees of Providence; and the glorious fabric he had erected,—that mental storehouse filled with the treasures of many years' collecting, fell to pieces before our eyes, and nothing remained but the broken fragments and the shattered scaffolding, to be again dispersed and scattered, without system and without order, until they should be again hereafter collected together with infinite labor and fatigue by some future master-mind.

The fate of Sir JOHN FRANKLIN has long been a mystery to his countrymen: he has probably long ceased to be a member of this Society. It is, however, only during the course of the past session that any authentic information has reached this country that the gallant explorer of the Arctic regions, with his adventurous followers, had ceased to exist. Far from their ships, which, in the extremity of danger and a hard struggle for life, they must have abandoned, and after vainly endeavoring to reach a more southern and hospitable region through a trackless desert, their remains were discovered by travelling Esquimaux, from some of whom portions of their property were obtained. These were rescued by the intrepid Dr. Rae, who had gone in search of them overland, and who brought back the melancholy certitude of their fate. Their bones now lie whitening on the Arctic shore, or beneath fields of eternal snow. By what means they reached that spot, or how they perished, will probably never be known; but their memories will ever be cherished as of men who risked and sacrificed their lives in the performance of duty and of scientific inquiry, and I trust I may also add, as the last installment of valuable lives sacrificed to a vain and chimerical attempt to discover that which, could it ever be discovered, would be alike unprofitable and unavailable.

Rear-Admiral Sir John Franklin was born at Spilsby in the year 1786, and performed his earliest service in the navy in the first year of this century, as a midshipman on board of the Polyphemus at the battle of Copenhagen. Sailing afterwards with Capt. Flinders to Australia, he acquired that skill in surveying and that power of observation which characterized his subsequent career. After serving in the engagement against Admiral Linois in the Straits of Malacca, he next acted as signal-midshipman of the Bellerophon in the glorious victory of Trafalgar; and, lastly, towards the conclusion of the great war, his gallantry was again displayed conspicuously in the naval attack upon New Orleans, for which conduct he obtained his lieutenancy.

A peace being established which promised a long duration, Franklin sought to be employed in the most adventurous service in which a seaman could then be engaged. He obtained, through the patronage of Sir Joseph Banks, the command of the surveying vessel, the Trent, being one of two ships under the orders of Capt. Buchan, destined to penetrate into the Polar Seas; on that occasion Franklin not only reached the high latitude of 84° 34″ N. lat. in the meridian of Spitzbergen, but evinced a strong desire to be allowed to proceed onwards alone, in the endeavor to effect a through passage.

The undaunted and inflexible perseverance which he exhibited in his explorations off the coast of North America, between the years 1819 and 1822, both inclusive, is well known to the public

through the clear and emphatic productions of his own pen. As geologists, however, we must specially remember, that the rock-specimens then brought home by Franklin, and his associate, the eminent naturalist, Richardson, first revealed to us the structure of those distant and inaccessible regions.

On his return to England, however, Franklin felt so strongly the want of better geological knowledge on his own part and on that of his officers, that when appointed to the command of the next Arctic expedition, on which he sailed in 1825, he took his first lessons in our science at the museum of our Society, accompanied by his distinguished companions, Back and Richardson. At these morning meetings our much-respected former President, Dr. Fitton, was the instructor, assisted by Mr. T. Webster, then our Secretary; Sir Roderick Murchison, who has informed me of these circumstances, being then also one of the learners.

The intimacy thus commenced continued till Franklin's last departure from the shores of Britain in 1845; for whether he was treading unknown tracts of North America, or commanding the Rainbow frigate in the Mediterranean, or performing the duties of Governor of Van Diemen's Land, our deceased member, having been knighted by his sovereign and duly honored by various public bodies, never ceased to correspond with his scientific friends, including Mr. Robert Brown, the enlightened botanist, and the old officers of this Society, Fitton and Murchison; sending to them also many specimens or descriptions which might, he thought, advance human knowledge.

As Sir John Franklin united the warmest heart and kindest manners to a solid understanding, it naturally followed that his friends took an intense interest in promoting all those endeavors to rescue him and his followers from their last perilous voyage, and in encouraging every effort directed to that end, whether made by the government or by the magnanimous Lady of the missing chief. The successive Presidents of the Royal Geographical Society, and particularly Sir R. Murchison, stimulated our rulers to make every possible research which might lead to the timely discovery of the absent voyagers. How some one of the earliest of these efforts might have succeeded, had it taken a southerly direction from Barrow's Straits, is indeed now established by the melancholy announcement made by Dr. Rae; for, although the party was supplied with provisions for three years only, we now know that a large remnant of the force had certainly sustained life for five years.

The late Professor JAMESON was the third son of Thomas Jameson, Esq., and was born at Leith on the 11th July, 1774. In his early years he showed a strong desire to become acquainted with natural objects, the study of which he evidently preferred to that of books and letters. His first attempts were made in stuff-

ing birds, and in collecting animals and plants on the beach of Leith and its vicinity. A strong desire to travel was the result of his favorite pursuits, and his father ultimately yielded to his often-repeated wish to enter on the profession of a mariner; but his friends interposed, and suggested that by adopting the study of medicine, he might equally be enabled to study the works of nature. He yielded in his turn, and was appointed assistant to the late John Cheque, Esq., surgeon in Leith. He commenced his study of natural history in 1792, under Dr. Walker, then Professor of Natural History in the College of Edinburgh, and soon became a favorite pupil. In 1793 he visited London, and became acquainted with the principal scientific men of the metropolis, and ever after spoke of the pleasure and benefit he had derived from his intercourse with Sir Joseph Banks, Mr. Dryander, Dr. Shaw, and other leading members of the Linnæan Society. With the exception of comparative anatomy, he now abandoned all idea of pursuing his medical studies. His attention was directed to the sciences of ornithology and entomology, then of chemistry, and subsequently of mineralogy and geology, including a thorough knowledge of analytical chemistry. In 1797 Prof. Jameson paid his first visit to the island of Arran, and in the following year he published his work on the 'Mineralogy of the Island of Arran and the Shetland Islands, with Dissertations on Peat and Kelp.' It was the first good geological account of these places and formations, and soon acquired a well-merited celebrity. He subsequently visited other portions of Scotland, and in 1800 published his 'Mineralogy of the Scottish Isles,' in two vols. 4to, illustrated with maps and plates. This work contained the first sketch of the geology of the Hebrides and Orkneys.

But the real period of Jameson's celebrity as a mineralogist and a geologist dates from the year 1800, when he left his native country for Freiberg, where he remained nearly two years studying mineralogy and geology under the famous Werner. Jameson fully acknowledged that it was from him he first derived clear and distinct views of the structure and classification of rocks. This opinion is confirmed by Conybeare, who says, "We are chiefly indebted to the reports of Werner's pupils, especially to those of Jameson, for our knowledge of Werner's general views, so fully developed in his lectures, and there only." Jameson also observed, in a passage which is too important not to be quoted on this occasion, pointing as it does to the very fundamental principle of all our modern geological investigations, that "Werner taught that mineralogical and geological characters, and characters derived from organic remains, were to be employed in determining formations, and that probably the same general geological arrangements would be found to prevail throughout the earth. But," he added, "the truth or falsity of this view in regard to the

similarity of formations, can only be determined by the united labors of geologists continued for a long series of years." This, it may be observed, is the very position our science now occupies, tracing out geological formations from one hemisphere to the other, referring the fossils of India to the age of the Chalk of England, and comparing the Palæozoic fossils of Australia with those of Great Britain and America. Thus, as Prof. Jameson admitted, it is to Werner that we are principally indebted for our present highly interesting views of the natural history of fossil organic remains; and in confirmation of this opinion, Prof. Jameson at a subsequent period vindicated the geognosy of Werner from the attacks made upon it by the Edinburgh Review.

In 1804 Jameson returned to England in consequence of the state of his father's health. Shortly afterwards, on the death of Dr. Walker in the same year, Jameson was appointed Professor of Natural History; and from that period, by his admirable lectures, founded in a great measure on the sound mineralogical and geological views of his friend and master the Professor of Freiberg, he raised the Edinburgh school of Natural History to the proud preëminence it has occupied for the last half-century. In the same year, he published the first part of the first volume of his 'Mineralogical Description of Scotland;' his other labors, however, prevented the completion of the work. In 1808 he founded at Edinburgh the Wernerian Natural History Society, of which he was elected perpetual President.

In 1809 he published the 'Elements of Geognosy,' a work which contributed more to introduce the doctrines of the Wernerian school into England than any other publication; and from this time may be dated the antagonism between the Wernerian and the Huttonian doctrines, as advocated by the northern geologists. Nor was the spirit of partisanship thus engendered altogether useless, inasmuch as its final effect was to call attention to the study of, and to diffuse a more general taste for, geology. Independently of this, the modification of the Neptunian theory as adopted by Werner, and in which form Prof. Jameson introduced it to the notice of his countrymen, has been proved by the test of modern science to be more consistent with the phenomena of Nature than the Plutonian views of its adversaries. It has served to introduce a more methodical study of the different formations of the earth's crust, in harmony with the numerous organic remains which they contain, and which never could have been reconciled with the doctrines of the Huttonian theory.

In 1813, at the suggestion of Professor Jameson, a translation of Leopold von Buch's 'Travels through Norway and Lapland in 1806, 1807, and 1808,' was published by Mr. Black,—Jameson himself adding to the interest of the work by an account of the author, and by various notes illustrative of the natural history of

Norway. In 1816, another edition of the 'System of Mineralogy' made its appearance in three volumes; and at the same time a new edition of his 'Characters of Minerals' was called for. Other editions of both works followed. In 1819, he commenced the 'Edinburgh Philosophical Journal.' For the first six years he conducted it with Sir David Brewster, but after that period he was the sole editor. It extends to seventy volumes, and is one of the most valuable repositories of scientific information in Britain. It will ever form one of the most durable monuments of his talents and industry.

But while Jameson was thus exerting himself in Edinburgh to propagate sound and correct views respecting the geological phenomena of the earth's crust, another distinguished naturalist was laboring in another capital to bring about the same results by the help of comparative anatomy.

In 1821, the immortal Cuvier published his 'Discourse on the Theory of the Earth,' as an introduction to his 'Researches on Fossil Bones.' To Professor Jameson we are indebted for the publication of a translation of this work made by Mr. Kerr. On this work Jameson observes:—"The notes I have added will, I trust, be found interesting, and the account of Cuvier's 'Geological Discoveries' which accompanies them will be useful to those who have not an opportunity of consulting the great work." This popular work produced an excellent effect in this country, for Cuvier was but partially known in England until this essay appeared. It rapidly ran through five editions: in the fifth, Professor Jameson entirely remodelled it, extending it from 190 to 550 pages.

During this period he also contributed many articles to the 'Encyclopædia Britannica' and to the 'Edinburgh Encyclopædia;' and on the return of Captain Parry from his Polar Expedition he drew up, from the specimens brought home, a sketch of the geology of the different coasts discovered and touched at by that enterprising navigator. But it would be occupying too much of your time, to enumerate the various works which flowed from his ever-ready pen. I cannot, however, conclude this notice without briefly alluding to one point respecting which Prosessor Jameson deserves the greatest praise, both for what he effected and for what he endeavored to effect. The present Museum of Natural History in Edinburgh is the result of Jameson's unceasing industry and efforts. The collections which existed before his time were almost entirely removed by the Trustees of his predecessor, Dr. Walker; and the nucleus of the present magnificent collection was Professor Jameson's private property, when he was called to fill the chair of Natural History. He labored incessantly to render it worthy of the place; but the means placed at his disposal, both by the Town Council and the government, were inadequate

to the task, and it was not without great private outlay that Professor Jameson raised it to its present state. In fact it may be said that the present Museum was founded, created, arranged, and exposed for public exhibition by the head and the industrious hands of Jameson alone. Professor Jameson died in Edinburgh, at the age of eighty, on the 19th of April, 1854.

The name of Arthur Aikin is associated with the earliest days of the existence of our Society. In that Charter which forms the basis of our constitution, his name occurs as one of the founders of this Society. He was born at Warrington, in Lancashire, on the 19th May, 1773. The grandson of John Aikin, D.D., eminent for his learning and abilities, he evinced at an early age a decided love for literature and science, and from his father derived a taste for zoology, for chemistry, and for English botany. An early acquaintance with Dr. Priestly, of whom he subsequently became a favorite pupil, and whom he assisted in the arrangement of a new laboratory, confirmed him in his predilection for chemistry. In 1797 he published an account of a tour in North Wales, made in the previous year in company with his brother Charles and another friend, under the title of 'Journal of a Tour in North Wales and part of Shropshire, with observations in Mineralogy and other branches of Natural History.' At a subsequent period, in conjunction with his brother, he delivered lectures on Chemistry and Chemical Manufactures, of which a syllabus appeared in 1799. In 1807 he published 'A Dictionary of Chemistry and Mineralogy,' 2 vols. 4to; and in 1814, 'An account of the most recent discoveries in Chemistry and Mineralogy.'

But before this time Arthur Aikin had become conspicuous as one of that distinguished band of scientific men who contributed to the formation of the Geological Society of London, and founded it in 1807; soon afterwards his knowledge of mineralogy and chemistry must have contributed to his being appointed one of the Secretaries of the Society. In the first volume of the first series of our Transactions, published in 1811, his name appears as one of the Members of Council. In the second volume, published in 1814, he appears as one of the Secretaries, as well as in the third volume, published in 1816; but there is reason to believe that he became one of the Secretaries at a still earlier period. In the first volume of the first series there is an interesting paper by him, entitled 'Observations on the Wrekin and on the Great Coal Field of Shropshire;' and in the third volume is another with the title of 'Some Observations on a Bed of Trap occurring in the Colliery of Birch Hill, near Walsall, in Staffordshire.' These papers, like all those published by the Society at that period, were of a much more mineralogical character than those now constituting the bulk of our publications. Palæontology had then made but little progress. Its value and importance in assisting our

knowledge of the relative ages of rocks was hardly recognized, nor amongst the illustrations which accompany the early volumes are there any figures of organic remains. At a subsequent period he was appointed to the Secretaryship of the Society of Arts; this circumstance is supposed to have led to his retirement from the office of Secretary to this Society; but he continued for many years longer to serve on the Council, of which he was a member for the last time in 1830. One of the earliest members of the Society who knew him well thus writes to me of him:—"He had a very logical head, and a calm and imperturbable temper, and drew up abstracts of the papers read at the meetings with a precision that might stand in comparison with those of Dr. Wollaston at the Royal Society." As an instance of character it is mentioned that in early life he had been a minister of the Unitarian persuasion, but resigned his cure on conscientious grounds. He was a corresponding member of the Academy of Dijon, &c. He died in London, on the 15th April, at the advanced age of eighty.

Dr. Stanger, the able and energetic naturalist of the ill-fated Niger Expedition, was born at Wisbeach, in Cambridgeshire, in 1812. He took his degree of Doctor of Medicine at Edinburgh, and subsequently visited Australia. He afterwards superintended, under the direction of the Government, the construction of roads near Cape Town, then returned to England, and settling in London, commenced the practice of his profession.

But the pursuit of natural history had greater charms for his enterprising character. In 1841 he joined the Niger Expedition under Captain H. Trotter, R. N., and was one of the few of that gallant but unfortunate band who were not struck down by the devastating fever of the country. It was mainly owing to his energy, assisted by Dr. M'Williams, that one of the steamers was brought down the river. In 1845 he was appointed Surveyor-General to the new colony of Natal, where, with the exception of a short interval of two years passed at home, he continued until his death. In this young colony his time was spent between the conscientious discharge of the duties of his office and a zealous investigation of the natural history of the district. But the pressing calls of his official duties did not permit of his reducing to order his many observations on natural history. One of his last contributions to botanical science, to which he was particularly devoted, was the discovery of a plant belonging to the family of the Cycadeæ, combining many peculiar characters, and named after him *Stangeria*. There is now a plant of it in the Royal Gardens at Kew, producing fruit. Exhausted by fatigue and cold, after travelling from Maritzburg to Natal on horseback, he died on the 21st March, 1854, and was honored with a public funeral as a mark of the respect in which he was held by the

authorities and inhabitants of the district. His loss is the more to be regretted, inasmuch as it disappoints those hopes held forth by my predecessor last year, in allusion to the geological discoveries to be expected from Dr. Stanger, who was to have undertaken an official geological exploration of the province of Natal.

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The only loss we have sustained amongst our Foreign Associciates is that of Dr. Gotthelf Friedrich Fischer de Waldheim, Professor of Natural History in the University of Moscow. He was born at Waldheim, in Saxony, on the 15th October, 1771, and studied mineralogy at Freiberg, with Leopold von Buch and Baron von Humboldt, completing his medical studies at the University of Leipzic. At Paris he subsequently attended the lectures of Cuvier, and carefully studied the natural-history collections of the French Museum. He had already given evidence of his extensive learning by numerous publications, when, in 1800, he was appointed Professor of Natural History at the Central School of Mayence. On his arrival there, however, he found that the chair had been given to another; and with that power of adaptation which belongs to true genius, he at once accepted the office of Librarian, which for a time led him away to other studies, particularly typographical antiquities. On this subject he published several valuable works until 1804. But he did not, in the mean time, neglect his favorite pursuit; he founded at Mayence a Natural History Society, of which he became the Secretary, and in 1804 published his 'Anatomie der Maki un der ihm verwandten Thiere.' In the same year he was appointed Professor and Director of the Museum of Natural History at Moscow, where a new field was opened to his talents, in which he had labored with zeal and energy during the remainder of his life. In the year 1805 he founded the Society of Naturalists of Moscow, and published the first volume of his 'Description du Muséum d'Histoire Naturelle,' the copper-plates of which he engraved with his own hands. This Museum, for the establishment and improvement of which he had so strenuously exerted himself, was destroyed during the conflagration of the city in 1812. Such a calamity would have gone nigh to overwhelm an ordinary man. Dr. Fischer rose above the circumstances, and with redoubled ardor immediately set to work to replace, as far as possible, the treasures which had been lost. Such were his efforts, and such was the success with which they were attended, that in a very few years the new Museum had again acquired a valuable collection of objects of natural history. He had now begun to direct his attention more exclusively to the study of fossil zoology, or as it is now called, Palæontology. In the 'Bibliographia Zoologiæ et Geologiæ' of Agassiz, published by the Ray Society, there are no less than 150 notices of separate works and memoirs in Journals and Transac-

tions published by him during the course of his long and laborious life. Among these are many bearing directly on our science, and which must have had considerable influence in directing the attention of the Russian Government to the mineral riches of the country, and of making its geological features better known beyond the limits of his own district, I will only mention a few of his more important works:—"Oryctographie du Gouvernement de Moscou," 1837; "Bibliographia Palæontologica Animalium Systematica," 1810; a second edition in 1834; "Notice des Fossiles du Gouvernement de Moscou," 1809–1811; "Notice sur quelques Animaux fossiles de la Russie," 1829; "Ueber verschiedene fossile Elephanten-species, die man unter dem Namen Mammouth begreift," 1831; "Recherches sur les Ossemens fossiles de la Russie," 1824; "Lettre à Murchison sur le Rhopalodon, genre de Saurien fossile du Versant occidental de l'Oural," 1841; Revue des Fossiles du Gouvernement de Moscou," 1846; and many others. He was elected a Foreign Member of this Society, and of the Linnean Society, in 1820. He died at Moscow, on the 6th of Oct., 1853, having nearly completed his eighty-second year.

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ART. XXXVI.—*Notice of Fossil Bones from the Red Sandstone of the Connecticut River Valley;* by JEFFRIES WYMAN, M.D.

THE fragments of bones here described were noticed more than twenty-five years since by Mr. Solomon Ellsworth while blasting for a well in East Windsor, Ct. The well had been sunk to the depth of twenty-five feet below the surface and eighteen feet into the sandstone, when some pieces of bone were recognised among the debris. They were examined by Prof. Nathan Smith and his account of them with a note by Prof. Silliman may be found in Vol. ii, of this Journal. In Vol. iii, p. 247, in a letter to the editor by John Hale Esq., it is stated that Dr. Porter who saw them when they were removed from the well, decided that they were not, as they had been supposed to be by some, human, but "belonged to some animal and that the animal must have been about five feet long. The tail bone was easily discovered by its numerous articulations distinctly visible, and by its projecting in a curvilinear direction beyond the mass." He also says that "they resemble some particular bones of the human body but would also compare with certain bones of other animals," but does not state what bones or what animal.

Through the kindness of Prof. Silliman and of Mr. Alfred Smith of Hartford, Ct., I have had all the specimens now remaining of the above mentioned collection placed at my disposal for examination. The collection consists of about sixteen pieces

of sandstone each containing one or more fragments of bone, but the larger number of these are so much broken up, that it was found impracticable to determine anything with regard to their nature. All are soft, chalky and friable; and in no instance is there one entire bone or one complete articulating surface visible. Yet imperfect as they are, coming from the New Red Sandstone, anything which can be determined with regard to any of them is of great interest. For while we have well preserved the innumerable impressions of the feet of birds and reptiles, the bones here noticed are, in so far as I can learn, the only ones which are publicly known to have been brought to light.

One of the best preserved bones is the fragment of a *vertebra*, the body of which is nearly entire, and is probably one of the "tail bones" described by Dr. Porter, but according to his description must have been more complete than it now is. The arch is broken off; the base of one pedicle remains; a transverse process on the left side is indicated by an imprint in the matrix; this process is broad at the base, thin and triangular, projects laterally in a horizontal plane and is slightly recurved. One extremity of the body is so completely imbedded in the matrix that only a slightly convex border is visible; the other extremity is exposed but is obliquely fractured, leaving only a part of the natural surface visible, and this is concave; the body is constricted in the middle, having an hour-glass shape, has no indentations on the side, and is somewhat compressed laterally. The floor of the spinal canal is represented by a deep channel, the sides of which meet at the bottom at an acute angle midway, but at either end spread out and become nearly level. The dimensions of the vertebra were as follows,

Length of body,	1·00
Breadth of body at extremity,	0·55
Breadth of body in the middle,	0·30
Length of transverse process (imperfect),	0·60
Breadth of transverse at base,	0·52
Breadth of spinal canal at narrowest part,	0·10

In front of the body just described is the fragment of what appears to be a second vertebra, and beneath this is a pointed fragment about seven-tenths of an inch in length, bifurcated where it touches the other bones and which has the character of an inferior spinous process. Behind the more complete vertebra is the anterior portion of a *third*, obliquely fractured so that a portion of its concave anterior extremity, and the edge of the left transverse process alone remain. In another piece of matrix is a portion of the arch and a cast of the spinal canal of a *fourth* vertebra, having also a portion of the spinous process; the canal conforms to that already described, and in front of this last vertebra is the fragment of the spinous process of a *fifth*. In another piece of

matrix is the fragment of a *sixth* having the articulating processes imperfectly preserved.

Of these six imperfect vertebræ only that first described, and the piece behind it offer distinctive characters of any importance. The presence of an inferior spinous process shows that it belonged to the caudal series; the existence of an anterior concavity, and the presence of a superior transverse process coexisting with an inferior spinous process are essentially reptilian features. In fishes the superior transverse processes are always deficient, and the inferior ones alone exist; these in the tail bend down to form the inferior spinous processes. If this bone is the one referred to by Dr. Porter as a "tail bone" his view as regards the kind of vertebra was correct, though he gives no reason for his conclusion. In addition it may be stated that it is a caudal vertebra of a Saurian reptile, to which it corresponds in the shape of the body and the transverse processes, and more nearly to those of the Crocodiles than any others.

Nearly all the remaining bones are too much broken up to allow of the determination. Still some of them present features of a general character, which are of sufficient significance to require a few remarks. One of the most striking characteristics is that of hollowness, and this in two cases is quite remarkable. In one of them we have a transverse fracture of what appears to be a cylindrical bone; the exposed fractured end is oval, measures one and one tenth of an inch in its long and six tenths in its short diameter; the cavity is very large, without cancelli and the walls of the bone are only one tenth of an inch in thickness. Near to this in the same mass of the matrix is another fragment which is exposed in transverse section as well as on a portion of its lateral surface. In its transverse section, it is more compressed than the preceding, but has equally thin walls and no cancelli. The whole fragment bears some resemblance to the upper extremity of the humerus of a bird,* distorted by pressure. The large size of the cavity and the thin walls lead us to compare this and the preceding specimen with the remains of birds, for it is among these alone that the cavities become proportionally so large and the walls so thin. Still it must be remembered that some reptilian bones are hollow. The Iguanodon, Pelorosaurus and Hylæosaurus described by Dr. Mantell, which had medullary cavities in the humerus and femur, but in them the walls were proportionally very much thicker, and the medullary canal was quite short.

Several other pieces of matrix contained fragments of bone which were hollow and with thicker walls; but they were too much injured to allow of determination.

* Its resemblance to a humerus was noticed by Prof. Silliman in the note appended to Mr. Smith's communication—in vol. ii, of this Journal

Two more pieces of bone remain to be noticed: they are about three inches in length, are longitudinally fractured and a large portion of each is gone; they have some indications of cancellated structure near one of the ends, and bear some resemblance to the bones of the fore-arm of a Saurian reptile, the form of the longer bone corresponding with that of the ulna, and the lower extremities of both agreeing in proportions with an ulna and radius; the upper portion of the supposed radius is nearly all gone.

Art. XXXVII.—*On the Distribution of Rain in the Temperate Zone;* by H. W. Dove.*

In the Meteorological Annual of France for 1850 observations for ten years on the quantity of rain falling in Algiers are given by Don, which show that the quantity diminishes almost regularly from January to July, and then regularly increases to December. This regularity is seen in the number of falls of rain, for in these ten years there were in January 88 rainy days, in December 83; in July, on the other hand, only a single one in 1844. These proportions hold good for the Canaries and Azores, they apply, too, even in the south of Europe, for in Funchal the quantity of rain diminishes from 92‴ in January to 0‴·9 in July. In St. Michael it is found after ten years of observation, to be four times greater in January than in July. In Lisbon the proportions are for December and July 55 : 2, in Palermo 37 : 2½, Naples has in March and October 49‴, in July not 7‴. Even in Rome the fall of rain is ten times more in October than in July. This periodical law is seen not only in the measurement of the fall of water, but in every accompanying phenomenon of temperature. After three months of almost perfectly clear weather only rarely interrupted by a tempest, the rains begin in Rome, on the 10th of October, though often sooner, and last, with fierce storms, almost without interruption, till the end of December. They diminish a little towards spring, so that the whole winter is rather a changeable than cold season, a continuous change from Tramontane to Sirocco. Even if we cannot divide the year, as do the Indians on the Orinoco, into a season of sunshine and clouds, yet the contrast of the rainless hot months and the continuously rainy winter is very marked. The beginning and end of this rainy period are usually marked by storms; Lucretius says of it:

> "Auctumnoque magis, stellis fulgentibus apta,
> Concutitur cœli domus undique, totaque tellus;
> Et, quom tempora se veris florentia pandunt:
> Frigore enim desunt ignes, venteique calore
> Deficiunt, neque sunt tam denso corpore nubes.

* Annalen der Physik, No. 94. Translated for this Journal by Dr. Rosengarten.

Inter utrasque igitur quom cœli tempora constant,
Tum variæ causæ concurrunt fulminis omnes.
Nam fretus ipse anni permiscet frigus, et æstum;
Quorum utrumque opus est fabricanda ad fulmina nobis,
Ut discordia sit rerum, magnoque tumultu
Ignibus et ventis furibundus fluctuet aër.
Prima caloris enim pars, et postrema rigoris,
Tempus id est vernum: quare pugnare necesse est
Dissimileis inter sese, turbareque mixtas.
Et calor extremus primo cum frigore mixtus
Volvitur, auctumni quod fertur nomine tempus:
Hæic quoque confligunt hyemes æstatibus acres.
Propterea, sunt hæc bella anni nominitanda."—*Lucretius*, VI, 357.

This description, which is exact for Italy, little suits our climate, the greatest number of rain-showers and the most storms occurring with us in the mid-summer months. But since the beginning and the end of the rainy period in Italy are marked by strong southerly winds, and a number of rules for the weather have come down from the olden time, which have their importance only over limited geographical distances, it hence seems to me not unlikely that the well-known equinoctial storms may still be admitted by us although in spite of the rule, we look in vain for them in our bright quiet September, and are not satisfied with the astronomical explanation of the postponement of the autumnal equinoctial to November or December.

The first explanation of this phenomenon is given by L. von Buch in his observations on the climate of the Canary Islands. "The Canary Islands," he says, "feel nothing that corresponds to the tropical rains, those rains, that is, which according to the sailors' phrase, follow the sun and fall when the sun reaches its greatest height. The rains of this climate first appear when the temperature falls considerably in winter, and the difference from the climate of the equatorial regions thus becomes greater and more strongly marked. The cause of these rains then appears to be the same that acts even to the pole,—the cooling of the warm air and the associated vapor which comes in from the southwest out of tropical regions and the lower latitudes, then, since this air in the autumn temperature of the Canary Islands is hardly cooled down to the condensation point of vapor, it is easy to understand why the rains begin much later here than in Spain or Italy, and yet earlier than in France and Germany. Rain is rare on the sea-shore before the beginning of November and does not often continue later than to the end of March. In Italy this period of rain lasts from the first half of October to the middle of April.

"Especially instructive and important for meteorological science," continues L. von Buch, "is the way in which the northeast trade wind is moved in winter by the southwest winds. These do not begin in the south and go towards the north, as would at first be imagined from their direction; but they reach

the Portuguese coasts sooner than Madeira, and Madeira sooner than Teneriffe and Canary; and so, as if from the north, these winds come gradually from above downward; and in these higher regions they are found even during the summer, while the north-east wind is moving over the face of the sea with considerable force. These higher winds descend slowly from the elevated atmosphere of the mountains. This is seen clearly in the clouds which in October surround the point of the South-Peak; which fall continually lower, and at last settle on the crest of the hills some 600 feet high, between Orotawa and the south coast, here to break out in fearful storms. Often a week, and sometimes more passes before they are felt on the seashore, for they last here for months. Rain falls on the outlying spurs of the mountains, and the Peak is covered with snow."

I consider the views thus expressed by von Buch, among the happiest thoughts which were ever brought forward in the science. It is the more to be regretted that in the year 1829, in his 'Essay on the Subtropical Zone,' L. v. Buch should have given these views an extent not sustained in nature. I know very well that formerly all naturalists supposed that the phenomena observed within circumscribed geographical limits, stretched similarly over the whole extent of the earth;—that they looked upon the monsoons as a local modification of the tradewind only within the Tropical Zone;—that they had no idea of finding in the Temperate Zone similar extensive differences under different longitudes, and of like geographic extent. I am far from charging von Buch with having created a subtropical zone that does not exist as a zone. Now however, that extensive observations over the whole surface of the earth are opening to us regions which were formerly beyond the reach of inquiry, it would be unjustifiable to blind ourselves to the new points of view that are offered us. It might sound paradoxical to say, that in summer the interior of Siberia has really such an absence of wind currents as we are in the habit of regarding as confined to the equator, but it must be declared a repudiation of physical laws, to say that the air of the Temperate Zone continually moves around the earth like a steady west wind, as is now printed in every text-book, for every one should know that the air moves only towards a region of disturbance, or after rising to such a place then turns back.

As proofs of the subtropic zone, Leopold v. Buch points out the yearly periodical diminution of atmospheric pressure from summer to winter, and deduces this diminution from the currents coming down from the mountains. Since in the Indian Ocean the southwest monsoon causes the barometer to fall, he takes this as one of the descending winds; for he says: In India, the southwest monsoons lower the barometer, and this just in proportion as they move downward. He adds further, that this periodic barometric

change does not extend across the Himalayah range, and that over these mountains another meteorological system begins; he extends this zone, making it to embrace the whole earth, beyond America to the Sandwich Islands.

In 1831 I wrote as follows, opposing the notion that the southwest monsoons are winds descending from above: (vide Pogg. Ann., xxi, 177,) "all observation seems to show that between the southwest winds on the outer limits of the monsoon and those in the Indian Ocean, the following differences exist: the former appear in winter, the latter in summer; those in a southerly direction are bordered by northeast winds, the others by southeast winds." According to Halley's theory, as followed by Mushenbroeck, Capper, Hube and Horsburg, these lower currents do not come from above downward, as conjectured by Halley, and proved by v. Buch.

The question why the appearance in the Atlantic is so different from that in the Indian Ocean, I then tried to answer by the theory that the high lands of Asia intercepted the passage of northerly currents, and thus no other current met the monsoon as it hurried up from below, except the mass of air between the neighborhood of the calms and those high mountains. With the approach of the sun, the mass of air moving in the perpendicular circuit diminishes, till at last it no longer opposes the southeast wind and takes quite an opposite direction. In the explanations of a phenomenon, all views may be divided into two classes, actual explanations, and evasions of nature. Of the latter kind was my former explanation, and so conscious was I of its weakness, that when the first Journal of Observations in Siberia appeared, I recognised and expressed the importance of its facts with reference to the theory of monsoons, pointing out that the barometric oscillation, which L. v. Buch thought was stopped at the Himalayah was greatest in the heart of the Asiatic continent, and that therefore the point of derivation of the southerly currents lay far towards the north.

Although the universality of the phenomenon of periodic diminished pressure was so clear that Kupffer in the edition of Asiatic Observations published in 1841, said: "one has only to look at the results of the observations contained in this volume to find a complete confirmation of all that M. Dove has advanced on this point," yet I then first proved decidedly that the southwest monsoons are the lower tradewind, which rushes toward the point of disturbance to fill up the void that owing to the increasing temperature here in inner Asia the diminished moisture was not sufficient to occupy. The proofs are indirect indeed, but all who have occupied themselves with meteorological observations know that hardly any problem of this nature can be directly proved, and that whoever has to do with the winds must follow the vane.

In a treatise that appeared in the Phil. Trans. for 1834, "On the Atmospheric Tides and Meteorology of Dukhun," Colonel Sykes showed that on the Plateau of Deccan, only 23″ of rain fell, which is about twenty per cent of the quantity at Bombay; but that the quantity is much more considerable on the sides of the mountain than in Bombay; and according to new observations in Mahabuleshwur, in fourteen years it amounted to the enormous quantity of 250″. In an essay contained in the Phil. Trans. for 1850, "Discussion of meteorological observations taken in India at various heights, embracing those of Dodabetta on the Neilgherry Mountains," he has further discussed the same phenomenon. Finally, last year's Journal of the Asiatic Society of Bengal contains observations at the military stations in Hindostan, from which it appears that in Cherraponjee on the slope of Cossya Hills at the same height as Mahabuleshwur, there fell the unheard of quantity of 610 inches in one year, an amount far exceeding that of any of the more elevated stations. A quantity increasing so extraordinarily at a certain height, which decreases however in places still higher, is quite irreconcilable with currents of air descending from above; it speaks more for a current moving horizontally against the side of the mountain, which is compelled to ascend on it, and then loses its vapor. I find a second proof of this ascent, in the fact that, (as I gather from some new calculations of the temperatures of Hindostan,) all the high stations show on the approach of the southwest monsoon a sudden increase of temperature, which is not found in similar proportion at any station on the level of the sea, a proof that those heights are swept by warm winds out of the valley, which climb up on them, for certainly the temperature curve of the higher stations must be more even than that of the lower.

From this, the immediate consequence is that the summer rains of the monsoon have nothing in common with the subtropical rains on the north and west coast of Africa. With the tropical they agree in succeeding the highest point of the sun's ascension; but they differ in not succeeding a '*courant ascendant*,' but an equatorial current, which is cooled at a higher level. The direction of the air-currents too is quite opposite. The tramontane rules in summer over the Mediterranean as a retreating protracted northeast current, at the same time the southerly monsoon prevails over the Indian Ocean. When the northeast monsoon reaches here in winter, the sirocco rages in the Mediterranean.

In 1835 I showed that the proportions of rain in the middle and northerly portions of Europe are comprehended from this simple point of view, that the time of winter rains on the outer limits of the tropics, the further we go from them, separate into two maxima united by slight fallings off, which come together

in South Germany into one summer-maximum, when the period of temporary rainlessness entirely ceases. This is observed especially where the current in the course of the year rises and falls considerably, and therefore at places, in a word, near the outer limits of the current, sometimes within it, and sometimes without it. It is clear that where this is not the fact, the changes in the temperate zone must be different. The delay of the current in the annual period is thus clearly accounted for.

As early as 1675, *Seller* laid down the inner limits of the northeast current in the Atlantic Ocean, very different from *Horsburg* in the early part of the India Directory. According to him, the zone between the two currents is fifty miles broad in winter, and one hundred and twenty in summer. Lately, Maury has tried in his large atlas, to ascertain the change of the northeast current for each month. He has shown clearly that this change is greater on the African than on the American side, or as I had before inferred, from the barometric phenomena, that the limit of calms and the associated phenomena of the current do not move in parallel lines but like a swinging thread, having its node-point in the West Indian sea, where the trade wind is a constant wind, and its greatest breadth of swing in the Indian Ocean, where it turns into the monsoon. How these extremes pass in the interior of Africa from one to the other, that is, how the current becomes a monsoon, we do not know, since most African travellers think that they have quite paid their tribute to meteorology, by complaining of the long continued heat there.

(*To be continued.*)

Art. XXXVIII.—*Correspondence of M. Jerome Nicklès, dated Paris, Sept.* 1, 1855.

"*Universal Exposition*" *at Paris.*—With the exception of Russia, the Industry of all Nations is represented at the Paris Exposition, but some more imperfectly than others, and none so poorly as that of the United States. The various machines, pieces of apparatus and products of which I have spoken in my communications to the American Journal are there on exhibition.

The ether machine of M. Trembley* is among them. The boiler is open to view. It has been rendered perfectly tight, by plunging the system of tubes which compose it into melted bronze. The ether vapor is thus completely confined and escapes only by the natural openings.

Boutigny exhibits his small boiler, by which the surface of evaporation is augmented without increasing the heating surface.†

M. Franchot has there his hot-air machine, as it was made in 1836.‡

The Arithmometer of M. Thomas de Colmar§ is presented on a large scale, adapted to calculate 30 numbers—which is beyond the wants

* This Journal, Nov., 1852. † Ibid., May, 1853. ‡ Ibid. § Ibid, May, 1855.

even of science. It is a very fine instrument as large as a common piano; and beside it there are others so small that they may be taken in the arms.

The Thermogenic machine of Mayer and Beaumont* is the object of unceasing curiosity; and it certainly is a remarkable fact that in this way, the simple friction of a piston of leather within a tube, making 300 turns of rotation in a minute, should furnish steam. The boiler is ordinarily under 2-horse pressure. It is actually put in use within the Crystal Palace, where it furnishes the hot water required for making coffee and chocolate. To produce the heat requisite for 2 horse power in the steam, requires at least 4 horse power. The inventors propose to use their machine only when it is convenient to utilise lost force.

In the fine display, made by Nachet, there are the multiple microscopes, of which I have spoken.†

Duboscq has on exhibition his electric regulator; Edmond Becquerel, his apparatus for chromatic photography; M. Felix Bernard of Bordeaux, a refractometer and also an apparatus for determining the polarization of the atmosphere.

Duboscq shows also stereoscopes by reflection and refraction. The stereoscopic views are less successful than those in the English Department.

Among the articles from England, we observe with interest the apparatus employed by M. Tyndall in his researches on magnetic phenomena; also the electro-magnet employed by M. Faraday for diamagnetic phenomena, and for examining the effect of magnetism on polarized light; this electro-magnet is in the show-case of Ruhmkorff, where there is also a differential galvanometer of Weber, and Ruhmkorff's induction apparatus,‡ which has proved of great service to amateurs in electricity. There is also a model of circular electro-magnets;§ another of trifurcate electro-magnets;‖ the rheostat of Wheatstone, etc.

M. Froment exhibits many kinds of apparatus of great interest:—as writing telegraphs;—a telegraph with a finger-board;—the gyroscope of Foucault;¶ apparatus for determining the velocity of heat, light and electricity, which has had the approbation of Arago, Bréguet, Silbermann, Froment, Foucault, Fizeau, &c. He also exhibits an electro-magnetic machine, which it is true will not be found particularly useful, but is still interesting on account of the plan of its construction. It is a circle of brass carrying at intervals iron armatures. This circle is placed in a drum also of brass, but furnished with horseshoe electro-magnets. On putting the current in action each armature in turn is attracted; and thus the interior circle is put in motion and acquires a velocity of rotation on itself, which is quite fatiguing in consequence of the shocks to which the circle is exposed.

In the French department, there are two other electro-magnetic machines: one moved by circular electro-magnets, the other with trifurcate magnets. Six circular electro-magnets with three poles are arranged on an axis. Around these electro-magnets, are arranged six cylindrical iron bars serving as armatures, and moveable upon their axes.

* This Jour., September, 1855. † Ib., Jan., 1855. ‡ Ib., January, 1853.
§ Ib., Jan., 1853. ‖ Ib., Jan. and May, 1853. ¶ Ib., March, 1853.

The circumference of the electro-magnets is at intervals terminated by brass plates so arranged as to serve to interrupt the magnetic circuit. As long as the iron portion of the circumference approaches the armature, it tends to separate itself from it, and carries in its movement the whole mass of the electro-magnet with the axis which supports it and the fly-wheel on the latter; but at the moment when contact takes place, the current is interrupted and driven through the electro-magnet, in consequence of which the play just described is recommenced. The apparatus is neither powerful nor economical. It would be better to employ circular electro-magnets with two poles.

The electro-magnetic machine with trifurcate magnets is more powerful, since the action between the magnet and armature is not exerted at the time of contact. But I may describe these machines at a future time with some details, and now add only that neither of them resolves the problem of an economical motor.

The only galvanic battery on exhibition, which has any novelty, is that called the electro-hydro-dynamic, by M. Chenot, in which he uses the sponge of iron, of which he is the discoverer, as already communicated. The same inventor has a machine which is an interesting application of electro-magnetism to the dressing of iron ores. These ores, being magnetic, are thus easily separated from the gangue and made ready for the furnace. The machine is a drum carrying electro-magnets on its circumference. A screen carries the impure ore under the drum where the poles of the electro-magnets take up the oxyd of iron and leave the gangue on the frame to be transported by it into an enclosure one side. As the drum moves on in rotation, the galvanic fluid is made to leave the first range of magnets and act upon the next; the ore of the first is thus dropped, while the next is taking its load. M. Chenot thus uses with profit ores containing only 9 per cent. of oxyd of iron.

Among the other applications which are on exhibition, not to speak of the magnificent galvanoplastic products, there are different systems of electric telegraph.

There is the signal telegraph of M. Bréguet, in which the same signals are made as sometimes used in the ordinary aerial telegraphing. It is in fact only a union of two telegraphs with ordinary dial plates; for if we suppose two of these telegraphs in action at the same time, and so arranged as to produce by their indicating needles whose extremities move over a black line drawn on the dial plate of the receiver, 8 positions in its turn in place of 26 which the alphabet requires, you will have some idea of the signal telegraph which has been adopted by the administration of telegraphic lines in France—adopted not without resisting progress for several years, and denying the benefits of the electric telegraph for a long period after this invention was employed in the United States.

M. Froment, exhibits a telegraph in which manipulation is through a box with a finger-board, having as many keys as letters. A arbor in this box tends constantly to move under a clock movement and carries in its motion the wheel which opens or closes the circuit; but it is retained by a catch which pressure on a key raises,—a simple arrangement for producing the breaks in the current. The receiver is that of the primitive telegraph.

The forms of telegraphic apparatus are numerous and interesting, though constructed on known principles. The more important are those of Froment, Bréguet, Garnier, in France, Berg and Soerrenden, of Sweden; the Company of the Eastern Railway, etc. The bell telegraph of Mirand has now many applications: its principle is that of Morse's telegraph.

Other examples of the application of electricity, are the Anemograph of Dumoncel, an instrument for measuring the direction, elevation, and force of the winds;* a heat regulator for hot-houses and drying chambers; and a system of electric communication between railroad trains.

M. Henley of London, has on exhibition a magneto-electric machine made of powerful magnets for springing mines. Deleuil, Breton, and Bekking of Holland, have induction apparatuses for medical uses. There is nothing especially remarkable in them.

The exposition contains also models of the submarine cables which have been used in connection with telegraphs, and of those which are soon to be laid down between Italy and Algiers.

In another communication, I propose to allude to other machines and chemical products and arts represented at the Crystal Palace.

Zoological Society of Acclimation. Project of founding a Museum of Applied and Comparative Natural History.—This zoological society continues its labors and with important results. Being essentially pacific in its objects, it has entered into relations with the Imperial Economical Society of St. Petersburg, and established with that Society a system of exchanges of objects industrially useful.

This Russian Society has in view an end long since proposed for France by M. Guérin Meneville, that is to make a collection of all raw products raised and used in the country, and of specimens illustrating the series of transformations they undergo in the processes of the arts. It would include also objects injurious to the articles which man cultivates. In such a gallery, the agriculturist, the trader, and the practical man, will find whatever illustrates his own departments, and be able to make comparisons important to direct them in the various operations of the arts and trade.

The St. Petersburg Society has already made such a collection of the productions of Russia, and their transformations. It includes all kinds of wheat, from the grain to the flour, bran, and starch, &c., as well as models of machines employed in cultivation, &c., from the plough to the wind-mill and the implements of baking. So there is wool of various kinds, and the raw material, and all the tissues coarse and fine, &c.; and so with a multitude of objects, pertaining to the three kingdoms of nature.

The Zoological Society of Acclimation, not wishing to confine itself to the productions of France alone, has resolved to reserve the objects it may obtain by exchange for a "Muséum d'Histoire Naturelle appliquée et comparée," to contain the products of all countries so as to exhibit the differences between them.

Acclimation of the Angora goat.—The Society is trying to decide whether the Angora goat can be acclimated. At their request, they

* This Journal, September, 1853.

have recieved from Abd-el-Kader, at Broussa (Asia Minor) a flock of goats from Angora, fifteen in number, which have been sent to the mountains of the south, centre, and east of France. We have seen some of them in the enclosures for acclimation of Dr. Sace at Wesserling (Haut Rhin), a valley possessing much that is curious in its industry, and of interest in the remains of ancient glaciers still traceable there. Two kids have been born since the goats were sent to Alsace, and as yet there is nothing to show that acclimation is impossible. The orientals doubt the success, and say that the texture of the wool changes even for small distances in the same zone and the same region.

The region occupied by the Angora comprises a part of Central Galatia and a southwest portion of Paphlagonia, between the parallels of 39° 20′ and 41° 30′, and the meridians of 33° 20′ and 35° east of Paris, the surface of which is about 2350 square metric leagues. The goat avoids the highest mountains, not ascending beyond 1600 meters, according to M. Tchihatcheff, who has studied the animal in Asia Minor. It also keeps out of the lower valleys where the heat is high in summer. The village of Angora is the place where this goat is raised with the most success and in largest numbers. Its altitude is about 1120 meters and its climate is liable to great extremes.

One of the most striking characteristics of the Angora goat, is its strong attachment to its native soil: a removal, however slight, causes a change in the quality of the wool. M. Tchihatcheff observes that all attempts to transfer it to Constantinople, Smyrna, and other parts of Asia Minor, have been without permanent success, the wool of the second generation beginning to deteriorate.

We may hope from the success experienced in other cases of acclimation, that Asia Minor is not the only place where the Angora may be made to live and flourish. The Egyptian goose was brought to France from Egypt in 1799 by Geoffroy, senior, and his son finally succeeded in acclimating it.

Since these trials with the Angora were begun, M. Graells, director of the Museum at Madrid, has stated that a flock of one hundred of these goats was introduced into Spain in 1830, and that there are now two hundred of them in the mountains of the Escurial, and still another flock in the mountainous region of Huelva.

Academy of Sciences.—The universal exposition at Paris has had some effect on the Academy of Sciences in bringing distinguished men from all parts of Europe and Britain to its meetings. At one recent session, there were present MM. Liebig, Brewster, Wheatstone, De la Rive, H. Rose, Poggendorff, Rammelsberg, Dove, Steinheil, etc. At this Scientific Congress, the United States is worthily represented by Mr. T. S. Hunt, of Canada, whose numerous confining occupations as a member of the Jury at the Crystal Palace, have hindered him from pursuing his scientific labors. He has made a series of communications to the Academy of Sciences, some of them treating on theoretical subjects of a profound character connected with great questions in philosophical chemistry now commencing to occupy the scientific world, and others specially on the geology of Canada.

Bibliographical Notices.—*Astronomie Populaire*, d'Arago, vol. ii, Paris: Gide and Baudry.—This volume is almost wholly devoted to the

Sun, the Zodiacal light and Comets. The theory on the physical constitution of the Sun; a careful essay on the Solar Spots; an examination of the Zodiacal light; a very complete treatise on Comets, give great interest to this volume. The author discusses the question whether the Sun is inhabited, and treats of the magnetic effects of the Sun on the compass.

He also describes the parallactic stand, the equatorial and the rotating dome of the Paris Observatory, as also the Polariscope lens of his invention.

Etudes et Lectures sur les Sciences d'Observation et leurs applications pratiques, by M. BABINET, Member of the Institute. 1 vol., 12mo. Paris, Mallet Bachelier. Price $2\frac{1}{2}$ francs.—This work is intended for popular reading. It contains several lectures, some of which were delivered at the periodical sessions of the five Academies of the Institute. One is entitled *Extraordinary Movements of the Sea*; another, *Comets in the nineteenth century*; another, *The Electric Telegraph*, in which M. Babinet states that he does not believe in the possibility of uniting Europe and America across the Atlantic Ocean, and thinks the course by Behring Straits the only one practicable; another, *On Astronomy in* 1852, 1853; *Aerial Perspective*; *The Stereoscope and binocular vision*; and finally it closes with an interesting chapter entitled "*Voyage dans le ciel.*" M. Babinet, a relative and friend of Arago, understands how to throw into his lectures the same charm and simplicity that characterised the popular public addresses of Arago.

Etudes sur la géographie botanique de L'Europe et en particulier sur la végétation du plateau central de la France; by M. H. LECOQ, Professor of Botany in the Faculty of Sciences of Clermont. 4 vols., 8vo, with plates. Paris: Baillière.—The author, who has for thirty years been exploring the mountains of Auvergne and the central plateau of France, has been led to compare the facts observed there by himself with those recognised by him in other regions. No one understands Auvergne like M. Lecoq; to him all questions are referred relating to the geology of that country.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the relations between the boiling points, specific volumes and chemical constitution of bodies.*—Since the appearance of his remarkable paper on the specific volumes of fluids, already noticed in this Journal, Kopp has published an elaborate memoir on densities, dilatations, and boiling points, confining himself however to a statement of his numerical results. In a still later publication he gives a brief summary of his conclusions which will be interesting to all chemists. With reference to boiling points the author finds a great number of facts which go to confirm the laws previously discovered by himself and already widely known. In a great number of comparisons of similar fluid bodies, the differences between the specific volumes taken at the boiling

points are found to be proportional to the differences between the formulas; thus the specific volumes of two different fluids differing in their formulas by x.C_2H_2 differ by nearly x.22. Isomeric fluids are found to have equal specific volumes. In numerous cases in which oxygen replaces an equivalent amount of hydrogen, the specific volume remains *nearly* the same as before the substitution. There appears however in these cases to be a real though small increase in volume. On the other hand, however, carbon can replace hydrogen in fluid compounds without producing a change of volume. The new researches confirm the results already obtained, that it is impossible, in a general manner, to deduce the specific volume of a liquid from its empirical formula, without regarding the direct results of experiment as inaccurate. It became therefore necessary to return to hypotheses on the rational constitution of chemical compounds and then the results show the uncertainty of the premises. The views brought forward can therefore only be regarded as presenting the simplest possible general expressions for the specific volumes of liquids. For this purpose Gerhardt's classification appears to be an advantageous one; the author's mode of representing the volume of the elements in fluid compounds is the same as that already employed by him (Ann., vol. xcii, 1,) but the numerical values of the specific volumes of carbon, hydrogen and oxygen are somewhat different from those formerly determined.

The specific volumes of fluids, which may be considered as derived from hydrogen $\left.\begin{matrix}H\\H\end{matrix}\right\}$ as type, correspond very well with the values deduced from observation if we take the specific volume of carbon as 5·5, that of hydrogen also as 5·5, and that of oxygen as 6·1.

The specific volumes of those fluids which may be considered as derived from water $\left.\begin{matrix}H\\H\end{matrix}\right\}O_2$ as type, may be satisfactorily deduced by giving the values above mentioned to carbon and hydrogen, but attributing to the oxygen, when it stands in water, as a specific volume of 3·9: 2 eq. of oxygen within a radical have the specific volume $2\times6.1=12{\cdot}2$, but 2 eqs. of oxygen outside of a radical have the specific volume $2\times3{\cdot}9=7{\cdot}8$. The same assumptions serve for compounds which may be deduced from multiples of water as types. For the determination of the specific volumes of compounds containing only carbon, hydrogen and oxygen, the author employed his own determinations, but for those of compounds containing other elements he made use of the observations of Pierre.

Sulphur occurs in its compounds in different ways, sometimes (1) replacing the oxygen in the type $\left.\begin{matrix}H\\H\end{matrix}\right\}O_2$ as in mercaptan, &c., sometimes (2) replacing carbon within a radical, as in sulphurous acid compared with carbonic acid, sometimes (3) replacing the oxygen within a radical, as in sulphocarbonic acid compared with carbonic acid $C_2O_2\left\{O_2=C_2S_2\right\}S_2$. In the first and second case the specific volumes of the sulphur compounds correspond satifactorily with the observations if we take the volume of sulphur as 11·3, but for the third case the specific volume of sulphur appears to be greater, namely, 14·3.

The specific volumes of a great number of chlorine compounds correspond well with those deduced by observation, if we assume the specific volume of chlorine as 22·8. In like manner, the volumes of iodine and bromine appear to be respectively 37·5 and 27·5. It is remarkable that bromine, as the author formerly conjectured, has the same specific volume, in its compounds at their boiling points, which it has at its own boiling point in the free state.

The specific volumes of but few compounds of other elements have been so far studied as to admit of accurate determinations of the volumes of the elements themselves. Phosphorus, arsenic and silicon appear to have the same specific volume, namely, 26. The specific volumes of $SnCl_2$ and $Ti\ Cl_2$ are equal, and suggest that tin and titanium have the same volume in their fluid compounds.—*Ann. der Chemie und Pharmacie*, xcv, 121.

2. *On a new class of Organic Radicals.*—Wurtz has succeeded in obtaining a number of double organic radicals, the existence of which had long been inferred, but which other chemists had not been able to produce. The author's investigations were undertaken in order to throw light upon the question whether the radicals methyl, ethyl, &c., have when isolated the same equivalents which they have in combination, or whether, as maintained by Gerhardt and Laurent, these equivalents must be doubled. If the molecules of ethyl, for example, on being set free unite with each other so as to form double molecules such as $\left.\begin{matrix}C_4H_5\\C_4H_5\end{matrix}\right\}$ then we should expect to be able to replace one eq. of ethyl in such a compound by one of some other similar radical, such as methyl for instance, so as to have a mixed or double radical like $\left.\begin{matrix}C_4H_5\\C_2H_3\end{matrix}\right\}$. Wurtz has obtained such compounds by two different processes. 1st, by decomposing by sodium an atomic mixture of the iodids of the two radicals. 2d, by the electrolysis of a mixture of the fatty acids of the series $CnHnO_4$. The author in the first place gives an account of the properties of butyl and amyl as prepared and examined by himself. Butyl is a colorless oily liquid which boils at 106° C.: its density is 0·7057 at 0° C. and the density of its vapor 4·070=4 vols. Amyl (long since isolated by Kolbe) is a limpid and mobile liquid, having a slightly aromatic odor; its density at 0° C. is 0·7413: it dilates strongly on passing from 0° to 20° when its density is only 0·7282. The density of its vapor is 4·956=4 vols.: it boils at 158° C. Amyl deflects the plane of polarization to the right, but its power of rotation is very different in different specimens, and the author finds that the same is the case with amylic alcohol, which, however, deflects the plane of polarization to the left. By the action of perchlorid of phosphorus upon amyl, Wurtz obtained two chlorinated products having the formulas $\left.\begin{matrix}C_{10}H_{10}Cl\\C_{10}H_{10}Cl\end{matrix}\right\}$ and $\left.\begin{matrix}C_{10}H_9Cl_2\\C_{10}H_9Cl_2\end{matrix}\right\}$.

The radical ethyl-butyl as obtained by the first of the processes mentioned above is a light and mobile liquid, boiling at 62° C. Its density at 0° is 0·7011; the density of its vapor is 3·053=4 vols. Its formula is $\left.\begin{matrix}C_4H_5\\C_8H_9\end{matrix}\right\}$. Ethyl-amyl has the formula $\left.\begin{matrix}C_4\ H_5\\C_{10}H_{11}\end{matrix}\right\}$, boils at 88° and

deflects the plane of polarization to the right. The other radicals obtained are butyl-amyl $\left.\begin{matrix}C_8\ H_9\\ C_{10}H_{10}\end{matrix}\right\}$, butyl-caproyl $\left.\begin{matrix}C_8\ H_9\\ C_{12}H_{11}\end{matrix}\right\}$ and methyl-caproyl $\left.\begin{matrix}C_2\ H_3\\ C_{12}H_{11}\end{matrix}\right\}$. The author concludes his interesting memoir with many very ingenious and attractive theoretical suggestions, for which, however, we must refer to the original.—*Ann. de Chemie et de Physique*, xliv, 275.

3. *On organic compounds containing metals.*—Frankland, to whom we owe much of our knowledge of this interesting class of bodies, has published an extended account of the results of his investigation of zinc-ethyl, which is doubtless one of the most remarkable of this class of bodies. To prepare this substance upon a large scale, the author employed metallic vessels (iron or copper) capable of resisting a strong pressure, in which the materials could be heated directly or after they had been placed in glass tubes. In this manner four to five ounces of zinc-ethyl could be prepared at a single operation. In one of these vessels granulated zinc and iodid of ethyl mixed with ether were placed, both dried with extreme care: the whole was then heated for 12 to 18 hours in an oil bath to 130° C. After opening the apparatus the contents were distilled off, the receiving vessels being filled with dry hydrogen, nitrogen or carbonic acid. Zinc-ethyl at ordinary temperatures is a colorless transparent very mobile fluid, of high refractive power and peculiar rather agreeable, smell. Its density is 1·182 at 18° C.: it does not freeze at −22°; it boils at 118°, and distills without decomposition. The density of its vapor was found to be 4·259. It consists therefore of 1 vol. of zinc vapor and 1 vol. of ethyl gas, the two condensed to one—

1 vol. ethyl gas,	2·0039
1 vol. zinc vapor,	2·2471
1 vol. zinc ethyl,	4·2510

The condensation in this case is very remarkable and seems to lead to the conclusion that the combining vol. of the vapor of zinc is not 2 like that of hydrogen. The author suggests that its combining vol. may be 1 like that of oxygen, and that zinc-ethyl vapor may therefore consist of 2 vols. of ethyl gas and 1 vol. of zinc vapor, the 3 condensed into 2 as in water. There is no instance known in which equal volumes of two gases combine with condensation. It appears therefore extremely probable that zinc and the metals belonging to the same group do not correspond to hydrogen as heretofore assumed.

Zinc-ethyl, though endowed with very powerful affinities, does not appear to be capable of forming true chemical compounds with electronegative elements. In the air it takes fire and burns with a brilliant blue green-edged flame, evolving dense vapors of oxyd of zinc, water and carbonic acid being simultaneously formed. When, however, zinc-ethyl is mixed with ether and a stream of oxygen passed into the mixture, the gas is absorbed and a white precipitate soon separates. The products of the oxydation here were found to be ethylate of zinc, acetate of zinc, and hydrated oxyd of zinc. Iodine acts powerfully upon zinc-ethyl, the reaction being represented by the equation

$C_4H_5Zn+2I=C_4H_5I+ZnI$. Bromine acts explosively upon zinc-ethyl, with formation of similar products. In chlorine, zinc-ethyl takes fire, the zinc and hydrogen burning, while carbon is set free. Dry flowers of sulphur gently heated with an etherial solution of zinc-ethyl unite with both zinc and ethyl and form mercaptid of zinc, C_4H_5S+ZnS. Water and zinc-ethyl instantly decompose each other, the reaction being $C_4H_5Zn+HO=C_4H_5, H+ZnO$: similar decompositions occur with the hydracids.—*Ann. der Chemie und Pharmacie*, xcv, 28, *July*, 1855.

4. *On the constitution of the ethers.*—Béchamp has presented to the Academy of Sciences a memoir on the action of terchlorid of phosphorus upon the alcohols and hydrated acids which possesses much theoretical interest. In a former paper the author shewed that glacial acetic acid behaves with terchlorid of phosphorus like a mixture of water and anhydrous acetic acid. In the present memoir he endeavors to show that alcohol is to be regarded, under certain circumstances at least, as a hydrate, and the compound ethers as salts of oxyd of ethyl. Very pure and dry acetic ether boiling at 74° C. heated in a sealed tube to 160°–180° C. during 24 hours gave a mixture of chlorid of ethyl, chlorid of acetyl and phosphorous acid. The reaction is here represented by the equation $3C_4H_3O_3, C_4H_5O+2PCl_3=2PO_3+3C_4H_3O_2Cl+3C_4H_5Cl$. Anhydrous acetic acid heated with the terchlorid of phosphorus yields chlorid of acetyl, the reaction being $3C_4H_3O_3+PCl_3=PO_3+3C_4H_3O_2Cl$. Very pure and dry ether heated with the terchlorid yields a similar reaction; we have $3C_4H_5O+PCl_3=PO_3+3C_4H_5O$. Hence the decomposition of acetic ether is precisely that of a mixture of ether and anhydrous acetic acid. The chlorid of phosphorus acts very powerfully upon absolute alcohol: the products are chlorhydric acid, chlorid of ethyl, phosphite of ethyl, and phosphite of water or hydrated phosphorous acid. The quantity of phosphorous ether is always very small, and it may be regarded as a secondary product, the alcohol being etherified by the muriatic acid. The equation will then be $3C_4H_5O, HO+2PCl_3=3HCl+3C_4H_5Cl+2PO_3$. The author concludes from these experiments that alcohol and the ethers of the monobasic acids are compounds of the same order and formed by the union of oxyd of ethyl with the anhydrous acid, since each one of the compounds which we suppose to exist in the alcohol or ether behaves as if it were isolated.—*Comptes Rendus*, xli, 23, *July*, 1855.

[Note.—It must be confessed that the author's reasoning is not very cogent, as all the reactions above mentioned are very easily explained by the so-called water theory, that is, by referring ether, alcohol, anhydrous acids and compound ethers to the type of water with the double equivalent, $\left.\begin{matrix}H\\H\end{matrix}\right\}O_2$. We shall then have the following equations:

$$3\left.\begin{matrix}H\\H\end{matrix}\right\}O_2+2PCl_3=3HCl+2PO_3+3HCl$$

$$3\left.\begin{matrix}H\\C_4H_5\end{matrix}\right\}O_2+2PCl_3=3HCl+2PO_3+3C_4H_5Cl$$

$$3\left.\begin{matrix}C_4H_5\\C_4H_5\end{matrix}\right\}O_2+2PCl_3=3C_4H_5Cl+2PO_3+3C_4H_5Cl$$

$$3\,{}^{H}_{C_4H_3O_2}\Big\}\,O_2 + 2PCl_3 = 3HCl + 2PO_3 + 3C_4H_3O_2.Cl$$

$$3\,{}^{C_4H_5}_{C_4H_3O_2}\Big\}\,O_2 + 2PCl_3 = 3C_4H_5Cl + 2PO_3 + 3C_4H_3O_2.Cl$$

$$3\,{}^{C_4H_3O_2}_{C_4H_3O_2}\Big\}\,O_2 + 2PCl_3 = 3C_4H_3O_2.Cl + 2PO_3 + 3C_4H_3O_2.Cl$$

W. G.

II. Miscellaneous Intelligence.

1. *Notices of two minerals from the Lancaster, Pa. Zinc mines*; by W. J. Taylor, (from a letter to J. D. Dana, dated Lancaster, Aug. 31, 1855.)—Allow me to bring to your notice two minerals which I have found in very small quantities at the mines of the Lancaster Zinc Company, a few miles northwest from Lancaster City, Pa.

The first one I will call Tennantite, until I find sufficient of the undecomposed mineral for a thorough quantitative analysis, which will decide the matter conclusively.

Color, steel gray. Streak, dark reddish gray. No crystals have yet been observed; fracture uneven; cleavage not observable in the small pieces found. A qualitative analysis in the wet way showed the presence of sulphur, arsenic, copper, iron and zinc.

The second mineral is Buratite or a variety of Aurichalcite. It occurs associated with calamine and partially decomposed blende, the rock being dolomite, through which there is interspersed occasionally copper pyrites in very small nodules. It is in radiated acicular crystals also laminated, also forming a coating on the limestone (dolomite) and quartz crystals which occur with it. Structure also columnar, which shows when broken the same radiated appearance. Lustre, pearly. Color, verdigris green. B.B. in a matrass gives out water which has neither acid nor alkaline reaction, and the crystals become brownish black. In the inner flame, yellow while hot, and white on cooling. With salt of phosphorus, intumesces and affords a green glass. With equal quantities of soda and borax a globule of copper. Effervesces with HCl, and dissolves entirely.

The ore from the mines of the Lancaster Zinc Co. is principally blende intermixed mechanically in a most peculiar manner with the dolomite rock, in which it is found; at and near the surface it has decomposed into a calamine, it is interesting to observe the process of decomposition very gradually going on.

2. *On Singular Cloud-belts, observed in Georgia, on the 13th of June, 1855*; by Wm. G. Williams, Prof. Nat. Sci., La Grange College, Alabama.—The arches, or belts of cloud observed in June 13th were seven in number, and appeared successively, beginning at about 9 o'clock in the evening. They were seen by me and others at Decatur, Ga. The following facts are from notes taken at the time. The first or northern belt, which may be regarded as the principal one, was first seen at a quarter past nine. It had then the form and appearance of a pillar of cloud, or of dark smoke extending upward from the horizon ten or fifteen degrees north of west, or a little south of where the sun set about two hours previous. At its base it was about two and a half or three degrees wide; its edges were not parallel, but gradually spreading like a

fan partially open. In form it was not unlike the tail of a comet. Its upper extremity was less darkly shaded, and was then at an altitude of about 30 degrees.

Presently we were surprised at its rapid extension in length, not however directly towards the zenith, but obliquely to the horizon along the zodiac, very nearly, if not quite parallel to the ecliptic, leaving the stars Regulus, and Spica Virginis, a little south of it, and finally in the east crossing the milky way considerably south of Altair in the Eagle. In about twenty minutes it reached the meridian, in fifteen or twenty more a point half way down the eastern sky, where it was met by a segment of an arch or belt precisely similar in its form, breadth, color and mode of progress, radiating from a point in the horizon, as near as I could judge, exactly opposite to the base of the western segment, or ten or fifteen degrees south of east, thus forming a complete cloud-like arch or belt, well defined throughout its whole extent; and, although less dense along its middle portions, yet everywhere so dark as to obscure entirely stars of the first magnitude. The darkest portions were about forty-five degrees from the horizon. Its breadth in the centre was about ten degrees, and its southern edge at the moment of its completion, which was about ten o'clock, not far from twenty degrees south of the zenith.

Meanwhile a second belt equally dark, and as distinctly symmetrical as the first, appeared in the west just south of it radiating from the same point, leaving an intervening space of clear sky of some four or five degrees. It was about half as broad as the first; but advanced eastward more rapidly, and in a few minutes made the whole circuit converging to the same point in the east, not however being met by an ascending segment. This was soon followed by a third. Then by another and another till seven were seen at once, each new one farther south, and narrower than the preceding, and leaving narrower spaces. Otherwise, in their general aspect similar, yet not so perfect and unbroken in their outlines. Moreover, although they all terminated in the same points mingling into one in the horizon, yet the last three or four did not seem to shoot forward as from a radiating point, as did the first three. These were rather formed in irregular segments like dotted lines or belts ultimately spreading longitudinally so as to unite.*

It was now half past twelve; but long ere this, to wit, a little past 10 o'clock, or just after the second belt was completed, I noticed that the northern, or original belt was moving laterally to the north, the whole rising upward like the bail of a bucket, on an axis having its poles in the two points of the horizon, where, as before remarked, all the belts terminated. It was followed by the second and third; and it was soon evident that the newly formed and still forming belts went rather to fill up the spaces left by the more northern ones than to advance with them. The rate of motion was apparently uniform, and such as to pass over ninety degrees in somewhat less than three hours.

In their progress, the first belt grew more distinct, and dense; the second less so, but quite variable. During its progress from the zenith to the North star, the former was more regular in its outlines and with

* The apparent radiation of these cloud-belts from the same point was in all probability optical,—that is, simply an apparent convergence in the distance of belts actually parallel or nearly so.—Eds.

its breadth somewhat narrowed in the middle, and its edges thus made much nearer parallel, it was more entire and deeper in its shades of darkness, than at any other period. During this interval the second fluctuated and varied greatly, especially near and just north of the zenith, and seemed to fall behind, spreading laterally, and becoming dim, and in parts, especially near the middle, appearing and disappearing and mingling with parts of the adjacent belts for a time; then again exhibiting distinct and regular outlines. More or less of these fluctuations characterised the other belts to the south during this time.

At half past twelve, or, about the time when the seventh and last belt appeared and arranged itself about 20° above and parallel to the *southern* horizon, then suddenly the original belt ceased moving, and became stationary, resting its lower or northern edge on a line parallel to the horizon, as nearly as I could determine, 20° above it. It now rested just as high and opposite to the seventh—all occupying 140°, or on an average for each belt and intervening space 20°. The first had revolved or moved laterally to give room for the others 90°, and now moved no farther. Its lower edge being still well defined and fixed, I saw a sudden and rapid expansion laterally of the upper edge, growing wider, and wider upward, till, within less than half an hour all the sky, as far as the zenith was filled, all the stars successively disappearing under its deep shades. All the belts now became much confused, and mingled with each other, which seemed to be a signal for their dispersion, an event now evidently taking place.

At one o'clock they were fast disappearing; first near the zenith, and along the middle of the successive belts; then in all directions, dissolving into thin air, leaving their places vacant before the eye. The original belt however, was still lingering nearly entire as late as half past one. Besides this, at that time, only fragments of the other belts remained in the west, and one in the east. There were not then, nor at any time during the whole of this strange phenomenon, were there any clouds visible, other than those forming the belts. On the contrary, the sky, as the belts dissolved, was unusually bright, and the stars everywhere shone with unwonted brilliancy as they also had before the commencement of the phenomenon, and indeed during its progress except in the belts.

Here my observations ended. I learn incidentally that fragments of the belts were seen at Atlanta, till, like clouds, they were tinged with a ruddy glow by the morning sun.

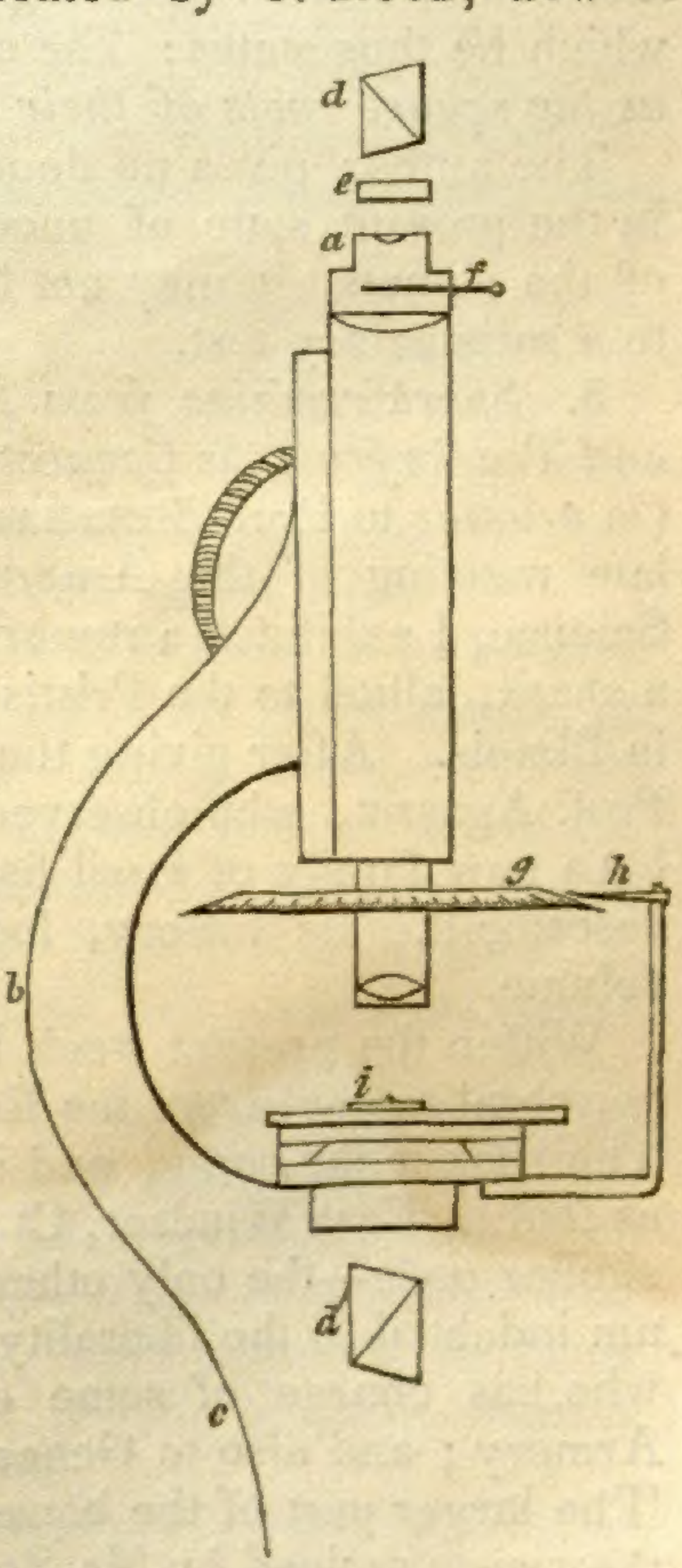
By way of supplement, and in order to throw some light on this subject, I here add the following facts and remarks.

(1.) The phenomenon was followed by a sudden and very great change of weather and atmospheric states as to temperature, moisture, and direction of the wind. For some days previous, it was fair, the air dry and cold for the season, dews generally light, wind north, or northwest. As a proof of these facts I may state that I heard the remark from one or more planters, that the weather was very fine for cutting and saving the wheat; but too cold for the corn to grow. Many were complaining of colds from the low temperature. Others, as myself, put on winter under-clothes. Indeed, on starting to church that evening, I felt the need of, and put on my cloak and wore it till the close of my observations, when I found my thermometer in its usual position

in the house stood at 67° Fahrenheit.* On the next day,† and for successive days, the heat was oppressive, wind south or southwest, air more moist and sultry, dews heavier, and weather unsettled with signs of rain. Beginning with the 17th, there have been here and all around copious showers daily, accompanied with much thunder and lightning, and sometimes driving winds,—that is, for five successive showery days,—the thermometer ranging night and day from 78° to 84°, with remarkable uniformity.

(2.) *It seems to have been local*, so far as yet heard from, and to have been seen farther along an east and west line than one north and south. It was seen in all directions from this place within 10 or 15 miles. It was seen in Cassville, 50 miles northwest. In Campbell Co., some 40 west, and in Newton. Also at Marietta, Ga., by Rev. Geo. White, Dr. Smith, J. E. Shelton, Esq., and many others, whose observation I find on comparison agree substantially with my own.

3. *On a combination of the Stauroscope and Compound Microscope proposed by Prof. von Kobell;* communicated by O. Rood, now of Munich.—In May last, a notice appeared in this Journal giving an account of the stauroscope, an instrument devised by Professor v. Kobell for the recognition of the crystallographical character of minerals and salts by certain optical means. Since then Prof. K. has combined this instrument with the compound microscope so that it is possible to subject microscopic crystals to the same examination as those of a larger size. The arrangement is quite simple and is adapted to any ordinary microscope which is provided with a revolving stage and goniometer circle, it is as follows (see figure‡): a Nicol's prism is attached to the under side of the stage, as in ordinary experiments on polarized light with the microscope; on the stage a glass plate holds the crystal to be examined; just above the eye-piece is placed a crystal of calc spar ground at right angles to the optical axis so as to show the rings and cross, and above this is another Nicol's prism.



The goniometer circle is attached to the *object-glass* by means of a ring fitting to both: to the under plate of the revolving stage which has no rectangular motion, is attached a vernier which will of course move in a circle around the vertical axis of the micro-

* Editor Watchman and Observer of June 21st says:—"Snow fell in Sewell mountain, Va., on the 12th inst."

† Same says in same paper:—"There was a heavy wind and hail storm in Rockingham, Va., on Sunday (17th June) blowing down trees and unroofing barns.

‡ In the figure, *a, b, c,* compound microscope; *d, d'*, Nicol's prisms; *e*, calc spar cut at right angles to the axis; *f*, divided micrometer plate; *g*, graduated circle; *h*, vernier secured to stage so as to turn with it; *i*, glass plate and microscopic crystal.

scope body and consequently of the goniometer circle which is attached to the object-glass; the crystal and vernier move together.

Within the eye-glass in the focus of the eye-lens is stretched a fine thread, or a divided glass micrometer can be used instead; the object of this is the same as that of the lines drawn on the brass plate to which the crystals are attached in the original instrument, viz., to bring a side of the crystal under examination into a certain fixed direction.

The mode of manipulation is the same as with the instrument first described; in this latter form however we have the advantage of seeing at the same moment the position of the crystal and its action on the black cross.

The original experiments were made with a Grunow microscope, which form is peculiarly well adapted for the arrangement described.

4. *Planetary Rotation.*—Just as this sheet is passing through the press we have received a newspaper slip from Prof. ORSON PRATT, Sen., dated G. S. L. City, Utah Ter., Aug. 4, 1855, announcing that he has discovered a law respecting the rotation of the planets on their axes, which he thus states: *The cube roots of the densities of the planets are as the square roots of their periods of rotation.*

The author gives no demonstration of the grounds of this principle. In the present state of uncertainty concerning the densities of many of the planets, it may not be practicable to bring the truth of the law to a satisfactory test.

5. *Shark remains from the Coal Formation of Illinois, and Bones and Tracks from the Connecticut River Sandstone;* by Prof. HITCHCOCK, (in a letter to Prof. SILLIMAN dated Amherst, 12th Oct. 1855.)—At the late meeting of the American Association for the Advancement of Science, I exhibited a remarkably fine specimen of a part of the jaw of a shark, allied to the Pristis family, obtained from the Coal Formation in Illinois. After giving the history of the specimen, I submitted it to Prof. Agassiz, who observed that it belonged not only to a new genus, but a new family of fossil fishes, and I have requested him to name and describe it. Its history, locality, &c., I hope to send for your next volume.

Within the present week I have obtained a portion of the bones of a vertebral animal from the Red Sandstone of Springfield, Massachusetts. The rock is the same, and its geological position essentially the same as that in East Windsor, Ct., from which you obtained some years ago similar ones—the only other instance known to me. For these bones I am indebted to the liberality of William Smith, Esq., their discoverer; who has charge of some excavations going on at the United States Armory; and also to General Whitney, Superintendent of the Armory. The larger part of the bones were thrown away by the workmen before they were noticed by Mr. Smith; but I have strong hopes that our anatomists may determine from those that remain, to what class the animal belonged.

Yesterday I procured a slab, weighing nearly a ton, from Roswell Field, Esq., of Gill, containing four gigantic tracks, apparently of a biped, yet accompanied by most distinct traces of a tail. It is certainly one of the most remarkable tracks which I have ever seen, and will probably, (as well as the bones above described,) throw light on the character of the animals that left the footmarks of this valley; I propose

for the name of the animal that made the tracks, that of *Gigadipus caudatus* (*γίγας* giant and *δίπους* biped; or biped tailed giant), of this, also, I shall hope to send a description for your next number.

OBITUARY.—Professor J. F. W. JOHNSTON, the distinguished Agricultural Chemist, died at his residence at Durham, England, on Tuesday the 18th of September.

[The following works have been received by us, but on account of the Index for volumes 11 to 20, here added to this number, we are unable at this time to give them extended notices, as well as to insert our usual miscellanies.—EDS.]

6. *Ranking's Half-yearly Abstract of the Medical Sciences.* No 21. Octavo, pp. 300. Philadelphia—Lindsay and Blakiston, June, 1855.—This admirable Semi-annual, contains a very large collection of valuable articles, selected from the current medical literature of both hemispheres, and is particularly full in the Departments of Practical Medicine and Surgery. It is conducted upon much the same plan as Braithwaite's Retrospect, with this exception—its covers furnish *no advertising medium* for *quack medicines.*

It must be an invaluable work for the practical physician whose constant occupation allows him but little leisure to keep himself well informed of the rapid progress which the Medical Sciences are now making.

7. *Elements of Natural Philosophy;* by W. H. C. BARTLETT, LL.D., Prof. of Nat. and Exper. Philosophy, U. S. Military Academy, West Point, author of "Elements of Mechanics," "Acoustics," "Optics" and "Analytic Mechanics:" IV. Spherical Astronomy, 466 pp. 8vo. New York, 1855. A. S. Barnes & Co.—The high reputation of Prof. Bartlett is a sufficient guaranty for the excellence of his works.

8. *Mathematical Dictionary and Cyclopedia of Mathematical Science*, comprising definitions of all the terms employed in Mathematics, an analysis of each branch, and of the whole as forming a single Science, by CHARLES DAVIES, LL.D., author of a complete course of Mathematics, and WILLIAM G. PECK, A.M., Assistant Professor of Mathematics, U. S. Military Academy, 592 pp. 8vo. New York, 1855; A. S. Barnes & Co.—This work has been prepared with care and is well illustrated with figures.

9. *Annual Report of the Superintendent of the U. S. Coast Survey for* 1854. pp. 4to, with a large number of maps and plates. This volume besides its beautiful maps, and its coast information, contains various memoirs of great value on Tides, the Gulf Stream and other subjects, by Prof. A. D. BACHE, Superintendent, and additional papers by different persons connected with the survey. Some of these chapters we may cite in a future volume of this Journal.

10. *Report on the Minerals and Mineral waters of Chile;* by J. LAWRENCE SMITH, Prof. Chem. Med. Depart. Univ. of Louisville. 25 pp. 4to.

11. *Esquisse Géologique du Canada, pour servir à l'intelligence de la carte géologique et de la collection des Minéraux économiques, envoyées à l'Exposition Universelle de Paris*, 1855; by W. E. LOGAN, Member of the Royal Society of London, etc., and T. STERRY HUNT, Member of the Geological Society of France, &c. 100 pp. 12mo. 1855. Paris: H. Bossange et fils.

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XVII.—Atomic Weights—2 charts: Cooke. p. 387.
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XX.—Universal Indicator for Microscopes: Bailey. p. 58.—On the Compounds of Zinc and Antimony: Cooke. p. 223.

Errata.

Vol. xi, p. 18, l. 16 from bottom, for "space," *read* "sphere."—P. 22, l. 5 from top, substitute $\int$ for *f*.—P. 23, l. 13 from bottom, for "any portion of *cgrs*," *read* "any portion *cgrs*."—P. 24, l. 15 from top, for $\frac{d^2P}{ds}d\Phi$ *read* $\frac{d^2P}{ds\,d\Phi}d\Phi$.—P. 33, line 8 from bottom, for πKR^2 *read* $2\pi KR^2$.—P. 34, l. 4 from top, for $\frac{d^2Q'}{dt}$ *read* $\frac{d^2Q'}{dt^2}$.—P. 97, 9th line from bottom, for *Lagostoma nodosa*, read *Lagostoma perlatus*.—P. 108, l. 12 from bottom, for "anilene," *read* "amilene."—P. 110, line 22 from bottom, for "$C_{14}H_5O_4 + 4HO$," *read* "$C_{20}H_6O_6 + 4HO$."—P. 112, l. 14 from bottom, for "urthethan," *read* "urethan."

Vol. xii, p. 17, 2d line from top, for "J. D. Macgowan," *read* "D. J. Macgowan."—P. 352, 16th l. from bottom, for "from four to five," *read* "about nine."—P. 354, 3d l. from top, for "five," *read* "eight;" 11th l., for "240 to 260," *read* "about 330;" 17th l., for "seventy-five," *read* "140;" 11th l. from bottom, for "fifty to sixty," *read* "70."—P. 356, 16th l. from top, for "two hundred and sixty," *read* "380."

Vol. xiii, p. 119, 4th l. from top, for "90° west of the Urals," *read* "90° east of the auriferous range of Australia."—P. 123, 6th l. from bottom, for "2dus," *read* "2do."—P. 218, 17th l. from bottom, for "grapes," *read* "grasses."—P. 372, 8th l. from top, for "peroxyd," *read* "protoxyd;" 16th l. from bottom, for "this city," *read* "Montreal."

Vol. xiv, p. 60, 2nd l. from top, for "C. Bechi," *read* "E Bechi;" 10th l. from top, for "Jargionite," *read* "Targionite."—P. 62, l. 20 from bottom, for "M : T," *read* "M or T."—P. 64, 12th l. from top, for "$6\ddot{A}l\,\ddot{S}i + 12\dot{H}$," *read* "$5\ddot{A}l\,\ddot{S}i + 9\dot{H}$."—P. 451, 5th l. from bottom, for "129," *read* "124."

Vol. xv, p. 297, 10th l. from top, for "Eocene Tertiary," *read* "Cretaceous."—P. 325, line 11 from top, for "northeastern," *read* "northwestern."—P. 358, l. 11 from bottom, for "place," *read* "work;" l. 18 from bottom, for "In one of his desultory volumes," *read* "In his 'Music of Nature.'"—P. 433, line 4 from top, for "*Sunadin*," *read* "*Sanidin*."—P. 436, 3d line from bottom, for "Thos. F. Seal of Philadelphia," *read* "Wm. W. Jeffries of Westchester."—In part of edition, p. 440, 4th line from bottom, for "ibid," *read* "J. f. pr. Ch., lvii, 276."

Vol. xvi, p. 250, 18 l. from bottom, for "*Melanipyrum pedicularis*," read "*Melampyrum, Pedicularis*."—P. 265, 15 l. from top, for "Trommei," *read* "Trommer."—P. 280, § 13, for "*Natur*," read "*steter*."—P. 289, 5 l. from bottom, for "passed," *read* "proposed."

Vol. xvii, p. 237, in part of the edition, line 10 from bottom, for "*O. enatica*," read "*O. erratica*."—P. 211, line 10 from bottom, for "Ca S," *read* "Cd S."—P. 220, line 7 from top, for "1 : 3 : 4," *read* "1 : 3 : 4⅓."—P. 240, to the analysis, *add* "phosphorus 0·21."—P. 287, line 9 from top, for "6 P. M." *read* "10 P. M.;" line 21 from bottom, for "59° 27," *read* "127° 9."—P. 333, lines 10 and 11 from bottom, for "Mr." *read* "Dr."—P. 431, 16th l. from top, after ilmenite, *add* "to calcite and other species."

Vol. xviii, the last line of p. 19 should be transferred to the bottom of p. 18. An error on p. 65 is corrected on p. 300.—P. 415, line 23 from top, *dele* "all but two of."

Vol. xix, p. 6, 21 l. from bottom, for "1852," *read* "1853."—P. 160, 6 l. from top, for "Gouch," *read* "Couch."

Vol. xx, p. 161, to the title *add* "by Prof. O. N. Stoddard."—P. 178, 19th l. from top, for "1838," *read* "1853."—P. 274, for "Nieder, Pobel," *read* "Nieder-Pobel."